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LECTURES

ON THE

Diseases of the Nervous System,

BY

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LECTURE XI.

On Contagion.

When diseases are propagated from one individual to another, either by actual contact or a near approach of the parties, they are said to be *contagious*, or to spread by *contagion* or *infection*; for these terms have often, and indeed generally, been employed synonymously: and the effect has been supposed to be produced by the application of some peculiar matter or *virus*, from the one to the other individual. Now there are a great many diseases that are obviously propagated in this way. Some are unattended with febrile symptoms; such as the *itch*, *syphilis*, and probably many more; which are termed, accordingly, the *non-febrile contagions*. Others are essentially accompanied with constitutional disorder, (*pyrexia*) and are called, by way of distinction, *febrile contagions*; such as *small pox*, *measles*, *scarlet fever*, and many more. Now it has been, and still is, much disputed, whether ordinary *continued fever*, in any of the forms that I have of late described to you, is to be ranked with *contagious* diseases, or, in other words, is capable of being propagated from one individual to another, in the manner of *small pox* and the rest of this class. The question is, doubtless, of much practical importance; as it involves the *prevention* of the disease; which, in the one point of view, would call for the adoption of measures of precaution that, in the other, would be not only superfluous, but might be positively injurious. The prevailing opinion has always been in favour of the *contagiousness* of *ordinary fever*, at least under certain circumstances. This general acquiescence on the point must, no doubt, rest upon something like proof; but whether the proof be satisfactory or not, merits a little further inquiry.

Discussions of this abstruse nature, require that the terms we employ should be accurate and well defined; a point that has seldom been sufficiently attended to. This uncertainty in the use of language is, I believe, the cause of much of the discrepancy that exists among practitioners on the subject before us. I shall state therefore at the outset, in what sense the terms *contagion* and *infection* are to be employed.

A *contagious* disease, then, is one that is capable of being communicated from one individual to another, in consequence of the application of some peculiar matter or poison, generated in the former and applied to the latter, either by actual contact or in the way of *effluvia*, it is immaterial which. Now the most perfect and striking example we have of a *contagious* disease, is the *small pox*; to which, therefore, we should constantly refer, as the standard of comparison. And the question then will be, is ordinary *continued fever*, as it appears in this country, for example, ever propagated in the same way as *small pox* is known to be, that is, by matter generated in the bodies of the sick? This question, which appears at first sufficiently easy to be answered, is, in fact, a very difficult one; of which we need no better proof, than the difference of opinion that still exists upon the subject. One would be almost tempted to believe that the question admitted of no satisfactory solution, or were in its very nature inscrutable. Such a conclusion, however, seems unreasonable; for the facts from which it is to be drawn are sufficiently tangible to admit of a rigid investigation. It is a mere question of testimony, the true value of which may be doubtless ascertained by cautious inquiry.

Now recurring to *small pox*, as the prototype or great example of a *contagious* disease, we find that it is propagated in one of two ways; either by the insertion of a fluid into a wound, by what is called *inoculation*; which fluid is obtained from certain pustules or vesicles formed in the skin, and which

possess a peculiar and determinate character; or else by *effluvia* that escape from the bodies of the sick, while labouring under the disease. Whether the effluvia are furnished from the matter lodged on the skin alone, or whether the exhalations arising from the lungs also, are capable of producing the disease, is, I believe, not clearly known. Now there is nothing similar to *variolous* matter to be observed in a case of ordinary fever. If, therefore, this disease be capable of being propagated like *small pox*, that is, if it be really *contagious*, it must be in the latter way, namely, by the effluvia arising from the bodies of the sick. In the case of *small pox*, the *contagious virus* may be considered as an *animal* production, formed, apparently, or *secretal*, as we call it, on the skin, under the peculiar inflammation that is there taking place. But you find nothing like this in fever; there is no obvious or external inflammation going on to which the formation of the *virus* can be ascribed. Here, therefore, is a failure of the analogy between the two diseases. Still it does not follow, that, under the peculiar morbid action that is going on in the system during fever, there may not be generated a *virus* that is capable of exciting a similar state of disease in others, and which may be thrown off from the body in the way of exhalation, so as to act at a greater or less distance. We cannot, however, have any demonstrative proof of this; because such effluvia, if they really exist, are not at all times, if at all, cognizable by the senses. It is alleged, indeed, and I think often with some foundation, that a peculiar odour is exhaled from the bodies of fever patients, by which, when concentrated, the presence of the disease may be known. And if this be true, it would go far to lessen the difficulty of supposing fever to be generated by the effluvia so characterised.

This kind of proof, however, can scarcely be considered as conclusive; and therefore we should inquire into the mode in which the disease is observed to spread afterwards; whether, for example, as in the case of *small pox*, a person labouring under ordinary *continued fever*, upon being carried to a distance, so that no local cause could operate to produce the disease, communicated the fever to others; and so on in succession, the disease still preserving its essential character. If this could be satisfactorily and unequivocally established, the point would be settled.* Now many persons have asserted positively, that they have known instances of this; and those are what we want, in order to decide the question. Unfortunately, when I have called for such proofs, I have generally found them deficient in circumstances, so as to leave some doubt upon the subject: there has been generally some other way in which the facts might be explained, and therefore, till we are furnished with more precise information upon the point, we are not at liberty to say positively, upon this ground, that a *contagious virus* is generated in the body in the course of ordinary fever, as it is in *small pox*. The proofs we actually possess, are of a less conclusive kind, though they are such as appear to be satisfactory to most minds.

It is frequently observed, that different individuals residing in the same dwelling, are attacked in succession by fever, having the same essential symptoms. Hence it has been inferred, that they communicated the disease one to another in the way of *contagion*, as the *small pox* would have done under similar circumstances. But then it is urged, that there might have been some common cause operating of a *local* nature, which might act sooner or later upon the different individuals, according to the greater or less disposition to be affected by it; hence that such an argument is not decisive on the subject. It is a well-known fact, that most of the medical officers of the *Fever Hospital* at Pancras, and almost every one of the attendants, have been attacked in turn by fever, sooner or later after their connexion with the Hospital; and many have fallen victims to the disease. This, it must be confessed, has much the appearance of *contagion*. In reply to this, however, it is observed, that the site of the Hospital is a very unhealthy one; and that the neighbourhood altogether, from its low and marshy situation, has always been prolific in fever of this description. But the disease is by no means so general in the vicinity of the Hospital, as such an opinion supposes: while the case of the *Small Pox Hospital*, which is immediately contiguous to the *Fever Hospital*, negatives such a supposition; for the inmates of the former show no such disposition to be attacked by ordinary *continued fever*.—Hence it does not seem reasonable to ascribe the disease in these cases to a *local* cause; in other words, to say that it is of *endemic* origin; and therefore the evidence still appears to be in favour of *contagion*.

Another explanation, and a more probable one, has been offered, namely, that the *secretions* and *exhalations* from the bodies of the sick, when accumulated by the neglect of necessary ablutions and ventilation, acquire a virulent property, so as to be capable of exciting fever in those coming within its range. And in confirmation of this, the example is quoted of fever of the most *malignant* description being generated by merely crowding together a number of persons, without sufficient attention being paid to cleanliness and ventilation, and that where none of the persons were at the time affected with fever. Thus is generated, as is

said, the *Gaol*, *Hospital*, and *Ship Fever*. The famous *Old Bailey* Fever is considered as a strong illustration of this, where the prisoners, brought into court for trial, infected a great number of persons of all ranks, but such only as happened to be placed in the direction of the current of air proceeding from the prisoners. The prisoners themselves were not labouring under fever at the time, nor was the disease then prevalent in the prison; but the prison being excessively crowded and ill-ventilated, noxious effluvia were generated, which, becoming attached to their clothes, were found capable of exciting fever in others exposed to them.

If this be the true origin of the *virus*, you may perceive a marked distinction between this and the case of *small pox*. In the latter case, the *virus* is the result of a *vital* action, a *morbid secretion*, like *syphilis*, and all the rest of the *specific* diseases. But in the former, the *virus* is formed external to the body, and proceeds from some decomposition or degeneracy of the natural secretions or exhalations passing off or deposited on the surface, the result of a *chemical* rather than a *vital* process. So that there is no proper analogy between the two. This difference justifies the use of a different term to denote it; and, accordingly, diseases propagated in this way, that is by noxious effluvia arising from the decomposition of *animal* matters out of the body, have been called *infectious*, to distinguish them from the really *contagious* ones, where the *virus* is generated within the body itself, by a kind of morbid secretion. In this view of the matter, ordinary fever may be said to be *infectious*, but not *contagious* like *small pox*. If this be correct, it is easy to imagine that *infectious virus* may be carried to a distance, by adhering to the skin or garments of a person, as well as by other ways. In this manner, the disease would *seem* to be communicated like *small pox*, though there would be a real and essential difference between the two.

But it is evident, that *prevention* would be far easier in the one case than in the other. In the case of *ordinary* fever, the *virus*, being lodged only in the clothing or other matters about the person of the sick, might easily be destroyed by purification; and the patient so purified might be removed without danger to those he afterwards communicated with. But in the case of *small pox*, and other real *contagions*, the patient is constantly carrying within himself a source of disease, capable of infecting others.

Now considering how numerous the exceptions are to the propagation of ordinary *continual* fever, by approach to the sick, and how rarely the disease spreads from one to another where ventilation and cleanliness are attended to, one is inclined to believe that fever is really propagated by *infection*, and not by *contagion*, like *small pox*, (employing those words in the sense above stated.) Thus, though ordinary fever may be denied to be *contagious*, in the sense that *small pox* is allowed to be so, it can hardly be denied that the disease is capable of being propagated from one to another in the manner stated, that is, by the decomposed *animal* matters accumulated around the bodies of the sick, but not actually generated within the body, as we suppose the *variolous virus* to be. This property of re-producing itself, possessed by common fever, is sufficiently well expressed by the term *infectious*; and if such be the case, it calls for means of prevention equally with *small pox*, though of a different kind. The prevention of *small pox*, for instance, requiring not only the destruction or purification of the clothes and other matters that have been recently in contact with the sick, but the avoidance also of the sick himself, however cautiously purified, and that for an undefined period, that is, as long as the *contagious* particles may be supposed to be present, and exhaling from the body. Unfortunately, the period during which this continues to be the case, is by no means ascertained; but it probably is not less than some weeks. Whereas, in the case of *ordinary fever*, it will be sufficient to cleanse carefully the clothes, &c. that have been about the sick, the person himself, when sufficiently purified by ablution, not being capable of communicating the disease.

Thus, I think, your duty on these occasions is sufficiently clear. You ought to act upon the supposition that fever is really *infectious*, in the sense I have stated: at the same time remembering, that where cleanliness and ventilation are attended to, the risk of *infection* is entremely small; and that the sick may be safely removed to another place, without any chance of communicating the disease, provided the person is properly washed and purified, and does not carry along with him the clothes recently worn. Now upon this view of the subject, you may reconcile some at least of the contrariety of opinion that exists in regard to the *plague* itself, (that great dread, and, as some choose to call it, *bugbear* of our legislators) and the necessity of the *quarantine* laws. If the *plague* belongs to the class of *infectious* but not *contagious* diseases, as seems highly probable, the practice of *quarantine* needs not extend to the person, but may be safely confined to the *goods* concerned, in cases where these do not from their nature allow of a satisfactory purification. By making this distinction, one-half of the oppressive effect of the *quarantine* laws would be obviated.

Of Complicated Fever.

Fever, in its *simplest* state, is an affection of the brain primarily, disordering by sympathy, like other inflammations, the general vascular system; thus giving rise to *pyrexia*, or a febrile state of body, but without actual disease in any other organ. In a great number of instances, however, it is found in combination with other inflammations, the seat of which is various, depending upon predisposition or individual tendency, climate, season, and other circumstances. These accidental combinations have led to erroneous conclusions, regarding the intrinsic nature of the disease; for they have been considered as proving, that no one organ is essentially or exclusively affected in fever, but that each in turn is equally liable with the brain to be so affected. This can only be judged of by attending to the symptoms of the disease, and the order of their occurrence; and if you do this, you will readily perceive that there is no foundation for such an opinion.

In the first place, in *simple* fever, there are no signs or symptoms present, but those which are directly referrible to the brain, and which consist in disordered feelings and functions of this organ. But in *complicated* fever, you have two sets of symptoms present at the same time, and which are easily distinguishable from each other; namely, those that arise out of the affection of the brain, and those which proceed from the organ secondarily and accidentally affected. You have, for example, the headach, the soft pulse, the brown tongue, the prostration of strength, and the tendency to delirium, that characterise *proper* fever; in combination with the local pain and disturbed function, of the part secondarily affected. Simple inflammation of other organs than the brain, is never attended with the symptoms of proper or *idiopathic* fever.

The secondary inflammation may arise at any period of the course of fever, or may even to all appearance begin with it. But it may also happen, that the fever is the *secondary* affection, the usual order in this case being reversed. Thus in simple *pneumonia*, it not unfrequently happens that during the course of the disease, the head begins to be complained of, the tongue is observed to turn brown, delirium makes its appearance, and there is extreme prostration of muscular strength. The disease is then commonly said to assume the *typhoid* shape, that is, it becomes a case of *complicated* fever.

Various parts are liable thus to become affected with inflammation during the course of fever depending upon the circumstances above alluded to. If there is in any individual a strong tendency to inflammation in any part, previously to the attack of fever, such part will be likely to fall into a state of inflammation during its course. On the other hand, the brain, in some persons, is so prone to inflammation, that fever, with delirium, and other appropriate symptoms, is apt to arise during the course of other inflammations, though of a trivial kind.

Climate has a material influence in determining the seat of such accidental *complications* of *idiopathic* fever. In hot climates, where a strong predisposition exists to abdominal disease, inflammation in the stomach or intestinal canal, is a very frequent concomitant of fever; giving rise to the enormous vomitings, often of grumous matters, (*black vomit*,) and to the dysenteric purgings, that so often appear as the attendants of fever in those climates. The *liver* is another part so disposed; and accordingly, this organ is frequently attacked by inflammation during the continuance of fever; and the *bile*, in some of these cases, being obstructed in its passage into the duodenum, and subsequently absorbed into the blood-vessels, occasions the yellowness that has given origin to the term *yellow fever*, a combination that is altogether accidental. Even in temperate climates, the abdominal viscera do not always escape. Indeed, I should say, that in a large proportion of cases of protracted fever in this country, inflammation arises either in the peritoneal coat of the intestines, or in the mucous membrane, the latter frequently resulting from the intemperate employment of cathartic drugs. Towards the end of long and hot summers, such combinations of *idiopathic* fever, with abdominal inflammation, are extremely frequent: and the *liver* partaking in the disease, its secretion is disturbed, and often greatly increased, so as to occasion vomiting or purging, or both. Such are usually termed *bilious* fevers. *Cholera morbus* itself, is most frequently a combination of fever, with inflammation of the mucous membrane of the whole alimentary canal. In the winter and spring, on the other hand, fever is found combined with the prevailing diseases of those seasons; such as *catarrh*, *pneumonia*, *pleurisy*, or *rheumatism*.

Fever is sometimes rendered more dangerous by such combination as I have mentioned, sometimes the contrary; the secondary affection, as it would seem, counteracting in some degree the disease in the brain, so as to reduce its violence. Thus, if inflammation arises in the stomach, or general structure of the intestine, or the peritoneum, in the course of fever, the danger is greatly enhanced; and to this cause, apparently, is chiefly owing the extreme fatality of the fevers of hot climates. Even with us, the occurrence of *peritonitis*

during fever, is a very unfavourable circumstance, and adds much to the danger of the disease: whereas slight diarrhœa, or catarrh, or rheumatism, exerts rather a favourable influence over the disease.

When these cases of *complicated fever* are examined after death, they present appearances that are in conformity with the symptoms that existed during life. Thus, in a great proportion of instances of persons dying of fever, the mucous membrane lining the alimentary canal, (a part very open to observation,) is found in a reddened, and sometimes ulcerated state; while, in certain parts of the canal, the inflammation extends to the general substance of the intestine, rendering it of a dark-red hue, thickened, and contracted. The frequent appearance of changes of this kind in the intestinal canal, has led to the belief that this is the essential seat of the disease; and upon this ground merely, the term *gastro-enteritis* has been applied to *idiopathic fever*; as if it were a constant or invariable occurrence. This, however, is not true. In *simple* fever, no such effects of inflammation are found in the intestinal canal: it is only where a manifest disorder in the actions of the canal existed during life, that these appearances are met with; so that, in fact, they may always be predicted. The frequency of such appearances in the present day, is ascribable to the almost constant and intemperate use of drastic purgatives, especially *calomel*, a medicine that is fully capable, in the three and five-grain doses usually prescribed, of exciting inflammation in the mucous membrane, which, in the depressed state of the system that exists in the advanced stage of fever, is very likely to prove fatal. In some of these cases, the brain has shown no appearance of inflammation when examined after death; or the appearances have been so slight, as to have been disregarded. The reason of this is obvious: the occurrence of the intestinal inflammation (which is always known by the symptoms of pain, purging, tension, and hardness of the abdomen) is often followed by an abatement, and sometimes an entire subsidence, of the cerebral disorder; showing a diminution of disease in the brain, and leaving little, therefore, to be discovered by dissection after death.

In treating *complicated* cases of fever, you are to consider what I have just stated, regarding the favourable or unfavourable influence of the *secondary* disease. If this should occur in a safe part, it should rather be encouraged, or at all events not hastily suppressed; as where *diarrhœa* arises in fever, it tends powerfully to mitigate the brain affection, while the hasty suppression of it, by *opium* more particularly, is productive of the worst effects. I have more than once had occasion to regret this, in my own practice: but where inflammation arises in the stomach, or intestines, or even their peritoneal covering, in the course of fever, it requires the greatest attention to be paid to it. The means to be used are, of course, such as are calculated to counteract inflammation in the part affected, but modified by the particular circumstances of the case. What might be proper in a simple case of *pneumonia* or *enteritis*, such as *blood-letting* to a large extent, might be wholly inadmissible in the same affection, when complicated with *idiopathic fever*. If the latter disease, for example, should have subsisted so long, as to have assumed the *typhoid*, or (as our Gallic neighbours call it) the *adynamic* state, (that is, where the brain is in a state of oppression, shedding its malignant influence over the general system,) the propriety of taking blood at all, is very questionable; but, at all events, it must be done, under such circumstances, with great caution and reserve. We are obliged then to rely upon remedies of a less efficient kind; such as leeches, fomentations, or blistering.

You will now, I presume, understand what is meant by the terms *erysipelatous*, *miliary*, *catarrhal*, *pulmonic*, *gastric*, and others, as applied to fever. They mean *idiopathic fever*, or inflammation of the general cerebral substance, combined with inflammation in some other organ, and which, therefore, I have called *complicated fever*; the combination, however, being purely accidental. Let us next consider

Periodical Fevers.

These are such as recur by *fits* or *paroxysms*, with intervals of different duration, in which the symptoms in a great measure, or wholly, disappear. *Periodical* fevers include both *intermittents* and *remittents*, which are, in fact, only different degrees of the same thing: the term *intermittent*, or *intermitting fever*, being applied where the intervals are nearly free from disease, while that of *remittent* is given to such as suffer a considerable abatement only of symptoms. That those are the same in nature, is evident, both from their arising from one common cause, and from their mutually changing into one another. I shall first speak of *intermittents*, as the most simple of the class of *periodical fevers*.

An *intermittent* then, or *intermitting fever*, which in vulgar language is called an *ague*, consists of two parts, the *paroxysm* or *fit*, as it is called, and the *interval* or time of *intermission*. The duration of these, respectively, and relatively to each other, is very different; the length of the *paroxysm* varying from four or six hours, to twelve or sixteen; but always terminating within twenty-four

hours. The *intervals* or *intermissions* vary much more; as from a few hours, to one or two days, or, as is said, even longer; still preserving a regularity of character. When the paroxysm recurs daily, the disease is termed a *quotidian*; when every other day only, a *tertian*; and when there is an interval of two entire days, it is called a *quartan*, that is, there are two paroxysms in four days. It is alleged that three, or four days may be interposed between the fits, or even a week; when the terms *quintan*, *sextan*, and *septenary*, have been applied: but this is less certain, the three forms I first mentioned being the only ones upon which we can rely as sufficiently established. Each of these has its proper time of attack; the *quotidian*, early in the morning; the *tertian*, about noon; and the *quartan*, in the afternoon. Any material deviation from this, is to be considered as an irregularity, and the disease is less manageable in consequence. These forms are frequently interchanged. Thus, if the disease begins as a *quotidian*, it often, after a few returns, becomes a *tertian*; and if it is prolonged for some weeks, it generally assumes the *quartan* form. But sometimes the order is reversed, or otherwise changed; the changes depending upon causes that we do not understand. There are other varieties occasionally observed in *intermitting fevers*: as the *double tertian*, where there is a paroxysm every day, but of unequal severity, those of the alternate days being alike; the idea in this case was, that two *tertians* were present in the system at the same time. We meet also in writers with the terms *semi-tertian*, *triple-quartan*, and many others; none of which are sufficiently established, as permanent varieties, nor are they at all understood: it is useless, therefore, to detain you longer with them.

Symptoms.—*Intermittents* often make their attack like *continual fevers*; but generally in a more sudden and violent manner; the *rigors*, or trembling with a sense of cold, being very severe. Many, however, begin with little or no *cold fit*. The duration of the *cold fit* is various; some times only half an hour; at other times, three or four hours, seldom longer. During the *cold fit*, which constitutes the *first* stage, the surface of the body and extremities is of a pale or sallow hue; the skin is corrugated, and the features contracted and sharp. The pulse is small, weak, and sometimes irregular. There is pain commonly felt at the back of the head, and down the back; the head feels as if tightly girded. If the head is grasped between the hands, an increase of heat is felt within, at the very time that the surface and extremities are cold. There is more or less of stupor and *insensibility* to impressions; the *mind* is weakened, and sometimes disordered; and the *muscular power* is irregularly, or, as we might say, *convulsively* exerted (*rigors* and *tremors*).—There is a sense of oppression or anxiety about the chest, and occasionally deep sighing.

These symptoms are gradually succeeded by those of the *hot fit* or *second* stage; the heat commencing at the centre, gradually extends to the surface and extremities; the skin becomes hot, but dry; the pulse strong, and frequent, and rather hard or contracted, though less so than in the *cold fit*; the pain in the head is of a throbbing kind; the face flushed, and the features tumid; the tongue is more thickly furred, and of a light drab colour. The sense of oppression at the *præcordia* now lessens; the tremors or shivering cease; the limbs are pained universally. If urine be passed at this time, it is scanty, scalding, and high coloured. The other secretions are diminished, or suppressed. The duration of this *second stage* of the disease, is from four or six hours, to eight or ten; when another change takes place, termed the *third stage*, or *crisis*, under which the paroxysm declines altogether. The skin becomes gradually moist, till profuse sweating occurs, under which the general distress of the patient lessens; the headach subsides; the pulse becomes soft and full; the urine more copious, and deposits a sediment as it cools. This is followed by sleep, from which the patient awakes comparatively without complaint: and in this state he remains till the next recurrence.

The duration of the disease altogether is uncertain. It may terminate after a few paroxysms, and that spontaneously; or it may be protracted to many months, during which it may undergo the changes in type I have before mentioned; sometimes it ceases altogether for a time, but is renewed again. On some occasions, after an unusually severe paroxysm, the disease never recurs; and in general it may be said, that a more violent paroxysm than usual, is followed by one of greater mildness.

Intermittents often begin like *continual fevers*, and go on so for many days; breaking down by degrees into the periodical form. In such cases, their real nature at first is not seen. Sometimes they *intermit* from the first; beginning, then, with a severe *cold fit*, or *ague*, as this stage of the paroxysm is vulgarly called. From being *intermittent*, they sometimes degenerate into an almost *continued* form; as when too early recourse is had to *tonics* and *stimulants*. Some have said that ordinary *continued fever* may become *intermittent*; but this is doubtful: it is at all events a very rare occurrence.

Intermitting fever is sometimes marked by the preponderance of some particular symptom, not altogether foreign to the disease, but unusual in degree. Thus we find in

authors, mention made of *tertiana phrenitica*, *soporosa*, *hemiplegica*, *asthmatica*, *hysterica*, *epileptica*, *tetanodes*, and many others. *Intermittents*, again, like *continued fevers*, may be complicated with other affections, generally inflammation of some organ. Thus, we have the *tertiana pleuritica*, *arthritica*, *urticata*, and others. Not unfrequently, we find it in combination with *dysentery*, or with *pulmonary* disease. Long-continued *intermittents* have a strong tendency to induce chronic inflammation in the *liver*, or *spleen*; rendering these organs hard, and often tumid: the indurated structure may then be often felt through the parietes of the abdomen, and is vulgarly termed *ague-cake*. The *liver* is often so affected as to obstruct the free passage of the bile into the duodenum, (the secretion of course going on.) Jaundice then makes its appearance.

Causes.—The principal, if not the *sole*, cause of *intermitting fever*, is the vapours or *effluvia* escaping from marshy soils, or any other situation where vegetable matters are undergoing decomposition by the combined agency of heat and moisture. The common term applied to these effluvia, is *marsh miasmata*. The disease is, therefore, of local origin, or of the class termed *endemic*, as I before explained to you. We have abundant proofs of its originating in this way, in various districts of our own country: as in the marshes of Essex and Kent; in the fens of Cambridgeshire and Lincolnshire; and in all other analogous situations. In Holland and the low countries of Germany, *intermittents* are still more rife, and more severe also: while, in similar situations in hot climates, they acquire the highest degree of malignity. The intrinsic nature, or chemical properties, of the *miasmata*, have not been ascertained. That they are formed by the decomposition of *vegetable* matters chiefly, can hardly be doubted, from the situation and circumstances in which they are generated; and also from the fact, that effluvia escaping from decomposing *animal* matters, give rise to fevers of a different character, in which the tendency to *intermission* is not in general, if at all, observed. These *miasmata* appear to be formed by the influence of heat on a moist soil. Hence, they are not observed to operate in winter, nor in the height of summer, when the surface of the ground is dry. The dense and fetid fogs that are seen to hover over the surface of marshes where *intermittents* prevail, are either the *miasmata* themselves, or more probably the medium in which they are suspended. These vapours appear to be limited in regard to the extent of their action, though the limits are not accurately defined. There is reason to believe, that they may be conveyed by the wind, to the distance of half a mile or more, in an active state. They do not appear to rise to any considerable height; for it has been observed that the upper stories of a lofty house were exempt from their influence, while the lower parts of the same dwelling, were affected by them. The activity of *marsh miasmata* is different in different situations, according as the combined influence of heat and moisture is greater or less. In this country, they are less active than in Holland and Flanders; while in Italy, as in the marshes near Rome, they acquire a high degree of virulence. They are by no means constant in their action, many individuals escaping their influence altogether, and enjoying excellent health in those situations: which shows that some predisposition is necessary to their effect. It is very remarkable, that while some persons are affected very quickly by marsh effluvia, as within a few days after exposure, others escape for a considerable time. And what is still more remarkable is, that persons are often exposed to them for six or eight weeks at a time, without being affected by them, yet upon removal to a distant and healthy spot, they often fall ill of *intermitting fever*, after an interval of many weeks, contracted apparently in the marshes they had quitted. General weakness of system, however induced, appears to be a predisposing circumstance: whereas a generous diet, warm clothing, and a moderate indulgence in the use of strong drinks, appear to afford a considerable degree of security against their influence. It is generally believed in Holland, that *smoking tobacco* contributes to preserve persons against the action of *marsh miasmata*; and this is not improbable, considering that *tobacco*, as well as *alcohol*, belongs to the class of *narcotics*, which exert a powerful and specific influence on the brain.

Theory.—You will perhaps be startled at first, Gentlemen, at my including *intermittents* with *continued fever*, as examples of brain affection, and as consisting in inflammation of this organ. This variety of fever has, indeed, been often brought forward as militating unanswerably against the doctrine which I have supported. I do not much wonder at this, when I consider the great influence of habit over our opinions as well as actions. We are so long accustomed to look on things in a certain light, that it is with difficulty, as well as reluctance, we are induced to change our views. I shall mention the grounds of the opinion I have adopted with regard to the *seat* and *nature* of *intermittents*, leaving the matter to your own judgment hereafter. I may observe, however, that the opinion has a material, and, as I believe, a beneficial, influence on practice; if true, therefore, it deserves to be maintained.

The *symptoms* of which the patient makes complaint, are, in all essential respects, the same in *intermitting* as in *continued* fevers. *Pain in the head*, often extending down the spine, is the leading symptom in both. There is in both, also, the same disorder observed in the *sensorial functions*, namely, in *sensation*, *voluntary motion*, and *intellect*; the latter being frequently disordered to the degree of delirium. In short, no one could distinguish the paroxysm of an *intermittent*, from a *continued* fever of the active or inflammatory kind; and it is, in fact, only by observing the termination and recurrence of the paroxysm, that the distinction can be made. Now as symptoms are nothing but the signs of an imperfect or disordered state of particular functions, they serve always to indicate the part immediately affected; or, in other words, to point out the seat of disease. An identity of symptoms, therefore, shows an identity of disease. Whatever, then, is true of a *continued* fever, in regard to either its seat or nature, is true of an *intermittent*, as far as the symptoms correspond. The principal difference between them, lies in the *periodical* character of the latter, and which does not belong to the other. Not only are the *local* symptoms the same, but the *general* also; *pyrexia*, or a febrile state of system, taking place in both. I do not believe that, strictly speaking, the *intermission* is perfect, that is, absolutely free from the disease; on the contrary, the tongue remains in a furred state, often with other febrile symptoms, though greatly reduced in degree. And as such symptoms, especially the furred tongue, are the consequence merely of local disease, (inflammation,) they serve to show that the primary disease, the brain-affection, still subsists, though so slight as hardly to disturb the functions. If you observe the tongue to be much coated in a patient recently labouring under *intermitting* fever, and there is no other inflammation existing to account for such a state of tongue, you may rely upon it that the paroxysm will recur again. But when the tongue has become clean, the patient is safe from a recurrence. In the intervals, therefore, of the most perfect *intermittent*, there is not complete *apyrexia*, or an absolute cessation of the febrile symptoms; nor, probably, has the local disease in the brain, wholly disappeared. It is an abatement of symptoms merely, and not an absolute cessation of them. And it is agreeable to this opinion, that there are all gradations between the regular *intermittent*, and the *remittent* form of the disease. *Dissection* is of less avail in detecting the seat and nature of *intermitting*, than of *continued* fever; and that for the obvious reason, that *intermittents* do not often in themselves prove fatal: when they do so, it is commonly in a secondary way, by inducing some other fatal disease, such as dropsy, or pulmonary consumption. Still, you will meet with numerous instances, in the writings of Morgagni and other morbid anatomists, of the most unequivocal signs of inflammation in the brain, in persons dying of *intermitting*, no less than of *continued* fever. The advantage derivable from this theory in regard to practice, I shall presently endeavour to show you.

Prognosis.—*Intermitting fevers* are not in general considered dangerous; nor are they so, under ordinary circumstances. It is chiefly by inducing other diseases, as dropsy or pulmonary consumption, that they prove fatal. Patients however have sometimes died in the *cold fit*, in a kind of stupor.

Treatment of Intermitting Fever.—As this form of fever appears producible only by a single cause, *marsh miasmata*, (other causes, such as exposure to cold and fatigue, acting as *concurrent* causes only,) it is not improbable that the disease is of a somewhat *specific* nature, and therefore not so immediately under the influence of remedies of a common kind, as in the case of ordinary diseases. If this be true, we must look to experience chiefly as our guide. Now the treatment of *intermittents*, as ascertained by experience, appears to be pretty well determined; though much advantage may at the same time be derived from the theoretical view I have taken, namely, that the disease consists in an inflammatory affection of the brain, modified in some degree, no doubt, by the nature of the exciting cause. In adopting such a view of the subject, you need not fear that I shall mislead you in regard to practice; for I am too well convinced of the importance and value of experience, to quit so safe and necessary a guide. But there may be cases where theory is a safer guide than what is called experience; for this is not always sufficiently precise and applicable, to be relied upon; persons in appealing to it, are often misled by loose and imperfect analogies.

The treatment of *intermittents* may be divided into that which is administered in the time of the *paroxysm*, and that which is employed in the *intervals*, for the purpose of preventing their recurrence; this last is of comparatively modern date, that is, since the introduction of the *Peruvian bark*; for before this time, *intermittents* were treated much like *continued fevers*, and chiefly by sudorific means. By forcing sweats, by the use of stimulant remedies and external heat, the paroxysm was for the time aggravated; but when brought to a termination, on many occasions it did not recur. Besides the uncertainty of this mode of practice, the case was sometimes rendered worse by the use of such active *stimulants*, and made to assume a *continued* form. But in general, the dis-

ease was left to follow its course, the remedies employed being of a very inefficient kind. The disease then often run a very protracted course, continuing for many months, and giving rise, in numerous instances, to the secondary affections I described to you above. The employment of the *Peruvian bark* for the cure of *intermittents*, was a great improvement in practice; for by its use in the intervals, the recurrence of the paroxysms is quickly prevented; so that the disease is cured often in the space of a few days.

Now in order to accomplish this object, it is necessary you should attend to certain circumstances, without which you will be very likely to fail in your attempts to cure, and very probably make the matter worse. The *bark* seldom succeeds in putting a stop to *intermitting fever*, unless the disease is tolerably regular in its character—that is, unless it recurs at the usual periods, and the *intervals* are free from disease, or nearly so. If, for example, there should be much head-ache observed in the *interval*; if the skin should be hot, the pulse quick, and the tongue more than usually furred; or if the disease should be complicated with another affection, such as thoracic or abdominal inflammation; the chances of failure with the *bark* are so great, that it is scarcely worth while or even proper to make the attempt, as the cure will rather be retarded than promoted by it. Your first object should be, therefore, to bring the disease to a regular form. This will often be done by merely leaving it to itself for a few days, undisturbed. The end may be more certainly accomplished by means of *counter-irritation*; such, as *vomiting* and *purging*, which ought in general to precede the use of the *bark*. *Sudorifics* are also applicable to the same purpose, though liable to the objection before stated, of being too stimulant in their operation.

Bloodletting is a powerful means of bringing an *intermittent* to a state of regularity, fitting it thus for the employment of the *Peruvian bark*, or similar remedies. Where the *intermittent* is complicated with another inflammation, either in the chest or in the abdomen, I consider *bloodletting* as almost indispensable to the safety of the patient. Now the notion so generally entertained, that *intermitting fever* is founded in debility, and therefore that *bloodletting* is adverse to the very nature of the disease, is totally without foundation. You need be under no apprehension on this ground. When used with due caution, I know, from frequently repeated experience, that it is not only safe, but of great advantage on many occasions, rendering the cure easier by other means. In numerous instances, I have found it take off the disease altogether, without any other assistance; the paroxysms becoming milder and milder, so as to cease wholly after three or four periods; the patient, at the same time, suffering little or nothing from weakness, in consequence of the loss of blood, but recovering his appetite, and quickly regaining his former strength. This I have witnessed over and over again, in the practice of this Institution, and in cases where the disease existed in the simplest form. The blood, in these cases, always exhibits the strongest inflammatory characters on its surface. Where the *intermittent* is found combined with *pulmonic* inflammation, or even *hepatic* disease, if at all acute, it must be an extreme case indeed, that would deter me from drawing blood. In thus acting, however, you are to remember, that the case is not one of active inflammation; nor in general is it of recent occurrence. The system, on the contrary, is often much reduced in point of strength, by the continuance of the disease. Large bleeding therefore is out of the question; but the taking blood to the extent of five or six ounces at a time, and this repeated at intervals of two or three days for as many times, you may rely upon it, is both safe, and, under the circumstances I have stated, highly beneficial. I am not contending, however, that *bloodletting* is always, or even generally, proper in the cure of *intermittents*. I think, on the contrary, that it should be confined principally to the cases where the disease is of a *complicated* nature. But it is of importance that you should know, that the practice is safe, and that the fears usually entertained with regard to it, are wholly groundless.

When, by the means now mentioned, the disease has become regular, it is in general easily put a stop to, by the *cinchona* or *Peruvian bark*; administered, not in the *paroxysms*, but in the *intervals* only. It should be as largely and as frequently given as the stomach will bear, without nausea; and it is more effectual, if employed in substance. For example, you may give from half a drachm to two drachms every second or third hour, from the termination of one paroxysm to the beginning of the next. When used with the precautions mentioned, the disease sometimes does not recur at all; and in general it ceases to do so after two or three fits. If the *bark* should produce either vomiting or purging, it fails to cure. The addition of some spice or spirituous liquor, makes it sit better on the stomach, and even increases its curative power very considerably. The active principle of this drug, lately introduced into practice by the French physicians, the *sulphate of quinine*, is a valuable discovery, on account of its greater efficacy, and more convenient exhibition. One, two or three grains, may be considered as an adequate dose. The

comparative value of the different species of *cinchona*, is not clearly ascertained; but it is certain that they all possess the same febrifuge property. Other *astringent* barks; and even the *mineral astringents*, as *alum*; and also the *bitter* plants, have all a power of preventing the recurrence of *intermitting fever*; though their efficacy is probably less than that of the *cinchona*. *Arsenic* has a wonderful property of the same kind, and appears to be more effectual than the *bark* itself. The usual dose is five or six drops of the *solution of arsenic*, as directed in the pharmacopœia; and it may be given three or four times in 24 hours, gradually increasing the dose to 15 or 20 drops. It is not necessary to confine its use to the *intervals*, as in the case of the *bark*.

Other modes of treating *intermittents* have been recommended; such as the giving a grain or two of *opium* at the commencement of the fit. Other *narcotics* have been used in the same way, and probably owe their curative power to the specific operation they exert on the brain. A large quantity of *strong spirit*, as half a pint of brandy, for instance, is a vulgar remedy. All these, however, are less certain, and are not at all times safe. *Intermitting fevers* are, in some degree, under the influence of the mind and imagination. *Terror*, for instance, has put a stop to the disease: and upon the same ground, *epithems* to the wrists, and *charms* and *amulets*, have now and then succeeded: but, of course, no reliance can be placed upon these. In obstinate cases, *mercury* appears to have subdued the disease after other means have failed, and is therefore worthy of a trial.

Remitting fever is but a variety of the *intermittent*; and, for the most part, an aggravated form of the disease. In hot climates, *marsh miasmata* generally excite the disease in this form. Thus in Holland and Flanders, where the heat is great in summer, though situated in a higher latitude than this country; and in the Mediterranean, as well as in warm climates in general, the majority of cases are *remittents*, the milder ones only being strictly *intermittent*. The *Gibraltar fever*; that which prevailed at *Cadiz* a few years ago with great violence; and also the *North American* or *Yellow Fever*; as well as that of the *West Indies*; together with various others, of all of which sufficient histories have been published, appear to be of *miasmatic* origin, and, as far as we can judge, are not *contagious*. It does not seem to be necessary, however, that the *miasmata* should proceed always from marshes; the effluvia arising from open sewers and drains, and the low and filthy parts of large towns, appear to produce fever with nearly similar characters; of which the *Gibraltar* and *New York fever* furnish good examples. In these situations, there is more of *animal* matter entering into the composition of the *miasmata*, which may probably modify their operation. *Predisposition* appears to influence considerably the action of these acriform poisons. Thus, it was observed in the celebrated expedition to Holland in the last war, that the British troops in great numbers were attacked with fever of the *remitting* form, and in the most fatal manner; while the natives, at the same period, were affected with ordinary *intermittents*, attended with little danger. This serves to explain also the extreme mortality of the *yellow fever* of the *West Indies*, to new-comers from a cold climate; while the natives, and those who had resided long in the West Indies generally escaped altogether, or suffered comparatively little from the disease. A very large proportion of these *remittent fevers* is accompanied with abdominal inflammation, upon which much of their fatality seems to depend.

These *remitting fevers* of hot climates, generally attack with great violence, and prove fatal in a very short space of time, often within eight and forty hours. So that nothing but the most prompt and active treatment can give the patient a chance of escape. It seems to be very well ascertained, that large and immediate *bloodletting* is the only means to be relied upon. Indeed there is hardly time for the operation of any other remedy. *Mercury* has been strongly recommended by some, while others have as strongly denied its powers in this respect. There is certainly something equivocal, in regard to the testimony that has been offered in favour of this medicine. It has been said, for example, that when *mercury* excited salivation in yellow fever, the patient invariably recovered. But then you are to remember, that in the worst cases of the disease, it could not be made to affect the mouth, in whatever quantity it was employed. Indeed in all fevers, there is a remarkable insusceptibility with regard to the action of mercury in the system; owing, probably, to the altered condition of the brain, and the change induced, in consequence, on the *irritability* of the system altogether: (thus affording, by the by, no weak argument in favour of the brain being the seat of the disease; for such an insusceptibility to the action of *mercury* is not observed in other diseases in any thing like the same degree). So that you see, that the patient who was salivated might have recovered merely because the disease was of a milder character in that particular instance; whereas, had it been of a severe kind, the mercury would not have acted at all.

We shall next have to speak of *Specific Fevers*, the *Exanthemata* or Eruptive class of Fevers, as they have been generally called.

FOREIGN DEPARTMENT.

PHYSIOLOGICAL RESULTS OF MODERN VIVISECTION.

The following observations are taken from an interesting little work, published during the past year, by Lund, in Copenhagen, entitled, "*Physiologische Resultate der Vivisectionen neuerer Zeit.*"

Experiments to determine whether the transit of the Semen into the Ovary be necessary to Impregnation.

This question has, as it is well known, given rise to tiresome, and very lengthened, disputes. Some considered that the transit of the semen through the fallopian tubes, was necessary for impregnation, founding such opinions on the recorded observations of Morgagni and Ruysch, who imagined that they had seen semen in the uterus and the fallopian tubes. Others, who as warmly denied such a passing over of the male fluid, referred to the opposite observations of many physiologists, of Harvey, De Graaf, and others, who assumed that the semen, either through a sympathetic agency on the vagina, through absorption, or through a certain aura, operated on the ovaries, and impregnated them. Without attempting to decide on these arguments, which are founded partly on speculation and partly on analogy, but which each party has deemed sufficient to establish its own opinions, I shall go at once to the examination of such results, as the experiments of modern times have afforded on this subject.*

Haighton† sought to solve this question, by cutting through the fallopian tubes of a rabbit, but in such a manner that they became re-united, and the wounds perfectly healed before being submitted to the male. He remarked, however, that the animal, after this operation, lost all sexual desire. He succeeded just as little at first with his experiment of cutting through one of the tubes; the animal for some time after showed no inclination for sexual intercourse. At length three of the rabbits on which this last experiment was performed, became pregnant; he opened them, and found in the ovaries of the sides not disturbed by the knife, *corpora lutea* and ova; in the ovaries of the injured side, on the contrary, he found corpora lutea, but no ova.

The experiments instituted by Blundell* agree with those of Haighton; he cut through the fallopian tube of a rabbit, but with the precaution, that it might again become united, he found that the ovarium of that side did not become impregnated. With the same care he divided the vagina near the orifice of the uterus, but the ovarium remained unimpregnated. From these experiments it appears clear, that whatever circumstance impedes the transit of the semen to the ovarium, prevents impregnation.

When, however, we consider the peculiar circumstances of these experiments, many things present themselves, which appear to render such a conclusion doubtful. Haighton and Blundell observed in the ovarium of the wounded side, corpora lutea, as perfectly formed as they appear after ordinary impregnation; the latter found that the fallopian tubes and the uterus had undergone such changes as are usual in incipient pregnancy, and that they were often considerably distended with fluid. According to all appearances, the ovum was really formed; but on account of the pathological condition of the parts produced by the operation, its development was impeded. Indeed the experiments made by Blundell, of cutting through the tubes in several places, but allowing them again to unite, after which operations the impregnation was not disturbed, show that the operation operated only as a mechanical injury, and in nowise restricted the formation and development of the ovum.

Another question which presents itself is, whether the unnatural closing of the tube and vagina did not operate prejudicially on the development of the ovum. A question which the experiments of Haighton seem to answer in the affirmative. He divided the fallopian tube in rabbits in six, twelve, twenty-four, and forty-eight hours after coition; if any immediate influence in general existed, one is justified in supposing that the semen would in this time of necessity have exerted such an influence on the ovary, since the vesiculæ of the same were quite distended with fluid, but in none of these experiments were the least traces of ova discernible in the ovary of the side operated on. But that the injury in these experiments only prevented the development, and not the formation of the ovum, appears evident, since Haighton, when he

* In this treatise I shall only cite such experiments as have been made on living animals.

† Philosophical Transactions, 1797. Part I. page 159, et seq.

* See Medico-Chirurgical Transactions, Vol. X. page 136.

instituted this experiment only a few hours after the last named period, during which time the ovum had entered the tube, found that it became there perfectly developed. We have, however, convincing arguments, that the cutting through of the trumpet may destroy the already formed ovum. We shall easily see how erroneous it is, from the afore-mentioned experiments, to determine whether we shall ascribe to them the prevention of the formation, or only of the development of the ovum. Since many of the phenomena observed in the above recited experiments, appear to indicate a previously existing formation of the ovum, the results obtained from these can only be regarded as evidences, and not as decisive arguments against the necessity of the transition of the semen into the ovary.*

* The later experiments of Dr. Blundell are quite decisive on this point, although they also prove that generation may, to a certain extent, be accomplished without any contact of the male semen with the rudiments. The fallopian tubes of rabbits were divided, first on the one side, and then on the other, and sometimes both at one time; the vagina leading to the one uterus was divided, and then the vagina of the second uterus; lastly, the vagina was divided lower down before its division, and the results were perfectly satisfactory. In both the uterine and vaginal experiment, the womb, though it contained no fœtuses, in many cases enlarged, as in extra uterine pregnancy. Its structure, too, became thoroughly developed, (speaking of the division of one vagina only,) it received more copious supplies of blood, in short, it frequently seemed as well prepared as its fellow, for receiving and cherishing the rudiments.

The ovaries, too, although there was no genuine impregnation of them, were very obviously excited. The vesicle in different parts of them germinated; its fluids increased; the delicate covering opened, the little cavity discharged its contents, and corpora lutea formed in all their perfection. As this appearance of the corpus luteum, notwithstanding the interception of the semen, is of considerable importance, and may help to clear away an objection to which the experiments lie open, it becomes necessary to examine it with attention. On the whole, then, it seems probable, judging from the appearances related, that generation may be carried forward to a certain extent, although the access of the semen to the rudiments be intercepted. Under these circumstances, the *young animal cannot be formed*, it is true; but *corpora lutea may* be generated; the wombs may be developed, and the *rudiments*, if we may judge from the facts already stated. may even be *transferred to the uterine cavity*, by the play of the fallopian tubes. —Ed. L.

On the Connexion between the Circulation of the Uterus with that of the Fœtus.

Innumerable injections, attempted without success, have shown, that no direct connexion exists between the arteries of the uterus and the veins of the fœtus.

Magendie, by his experiments on living animals, arrived at the same results. He injected poisonous and colouring substances into the vessels of the uterus, without having once observed any such appearances as allowed him to believe in the immediate transit of the same into the vessels of the fœtus. After he had injected camphor into the veins of a dog, he allowed the circulation to go on four minutes undisturbed, and then took out a fœtus from the uterus: the blood of the fœtus did not smell of camphor; in a quarter of an hour he took out another fœtus, the blood of which smelt very strongly of this substance. From which it is clear, that if no immediate, at least a mediate transition exists, probably through the deposition of the arteries of the uterus, and the absorption by the veins of the fœtus From the fœtus to the mother, no transit appears to exist. Magendie has often injected the most powerful poisons into the arteries of the umbilical cord, without being able to detect the least trace of them in the uterus.

On the Reproduction of the Nerves.

The question, whether the nervous substance is really reproduced, has for a considerable time occupied the attention of physiologists. Numerous experiments have been tried on the subject, by the physiologists of the last century, but the question at the commencement of the present century remained unanswered; and notwithstanding the united exertions of anatomists, physiologists, and chemists, it cannot yet be considered as definitively settled. Galen endeavoured to determine the point physiologically, by observing in what degree the function was restored; the greater number of his experiments were attended with an unfavourable result. In the year 1776, Cruikshank entered on the same field of investigation. After having been convinced that the division of the par vagum on each side invariably produced the death of the animal when even one of these nerves was divided a week later than the other, he prolonged the interim to that of a month, and the animal still lived; on examination after death, he found that the nerve which had first been divided, was united by means of

a filament, much thinner and less fibrous than the rest of the nerve. This regenerated substance in Cruikshank's preparation, was not, however, looked on as really nervous; the two Hunters, and afterwards even Cruikshank himself, entertained doubts on it. In the year 1778, Fontana * saw this preparation in London, from which he was induced in that and the following year to try some experiments on rabbits respecting this point. He removed from the ischiatic nerve, the par vagum, crural, and intercostal nerves, portions from 6 to 8 lines in length, and observed in two cases where he had cut away 6 lines from the par vagum and intercostal nerves a real union by nervous substance, the nature of which he was assured of by microscopic observation. After him Monro † set on foot some experiments with the division of the ischiatic nerve and spinal marrow on frogs. The regenerated substance did not appear to him quite to accord with nervous substance, and the function was never perfectly restored. Soon after this Michaelis ‡ was induced by the same preparation of Cruikshank's, to try some experiments on the same subject. In the years 1782 and 1783, he tried eight experiments chiefly on dogs, six with the phrenic and two with the ischiatic nerves. He remarked after the space of a few months, and sometimes a few weeks, in six cases a real and perfect nervous substance which united the mass; two experiments did not succeed, but the cause of failure he attributed to himself. At last Arnemann § appeared as the opposer of nearly all those who had preceded him. From his numerous experiments, he concluded that the regenerated substance was never medullary matter, but only thickened cellular membrane; that when the function was restored, this was only effected by the cellular membrane during its gradual contraction, bringing the divided ends of the nerves in contact. Arnemann's view gave to the subject a quite different turn, till Haighton, ‖ in 1795, by his experiments confirmed Cruikshank's opinion. He observed that a dog whose par vagum on both sides was divided at one time, died eight hours after, but that if there was a space of three days between the division of the two nerves, the animal expired within the fourth, and if of nine, within the thirteenth day. The operation, however, in all these cases was fatal, and death followed sooner or later in proportion to the space between the division of the two nerves. But on extending the distance of time to six weeks, the animal survived both operations, and within six weeks was perfectly restored. In so far he had certainly confirmed Cruikshank's results, but as the objection might be made to both, that the substance of the divided nerves had possibly not been reproduced, but that their functions had been carried on by other nerves, he divided, after the animal had recovered a second time, the eighth pair, which in such a case must have been useless, but the animal died on the second day after the operation.

Up to this period those weapons only had been used in the determination of this question which anatomy and physiology afforded; now, however, the assistance of chemistry was consulted. Reil * had discovered a new method for distinguishing the true nervous substance from the neurilema and cellular membrane, viz. by corrosion with nitric acid, by which the one remained unacted on, whilst the others were dissolved. Meyer † employed this method in order to determine the question on the reproduction of nerves. He found that the ends of a divided nerve really united in the course of a few weeks by thin threads which could not be dissolved in nitric acid. Even if he removed part of the substance of the nerve, its ends even then united, but the union took place slower; for instance, when he removed a line he found that reproduction was effected in three weeks, and when two lines, after two months.

Although from these experiments it might appear that the question was fully settled, we still find some distinguished physiologists opposing the idea of reproduction of nervous substance. Thus, for instance, Richerand ‡ states, that he was sufficiently decided to adopt the opinion that the nerves were reproduced, but that he repeated Haighton's experiments without success. The paralysis, according to this author, which followed the division of the nerves,

* Richter de vulnerum sanatione. Tubing, 1812.

† Monro's Observations on the Structure and Functions of the Nervous System.—p. 81.

‡ Michaelis' Brief an Camper uber die Regeneration der Nerven, Cassel, 1785.

§ Arnemann Versuche über die Regeneration der Nerven. Göttin. 1782.

‖ Haighton in the Philosophical Transactions. 1795. I. p. 190.

* Reil's Exercit. Anatom. de Structurâ Nervorum. Fas. I.

† Meyer, Reil's Archiv. 2 ter Band. S. 449.

‡ Richerand Nosographie Chirurgicale. Tom. II. 207, 210.

is incurable. Delpech* states the same thing, and also denies the reproduction of nerves. Magendie† also affirms, that he frequently repeated Haighton's experiments; the animals, however, invariably died five days afterwards, and from this he supposes, that Haighton must have committed some mistake about the division of the nerve. Owing to these contradictory statements, Joseph Swan‡ was induced to repeat all the experiments anew. On rabbits, he tried twenty, and on dogs two, of which the following are the results. After the division of a nerve, the extremities, particularly the superior one, became thicker and more vascular; coagulable lymph, having the appearance of albumen, was poured out, and in a very short space of time permeated by blood-vessels. In the course of a few days, this coagulable lymph united from both ends, and anastomosing vessels were soon formed in it. It gradually acquired a firmer texture, the number of blood-vessels diminished, it shrunk together as in cicatrization, and the separated extremities approached each other more; but it was difficult to determine when the nerves were capable of again resuming their functions. One rabbit recovered itself a little within eight weeks, but in the 18th it was not quite restored. This is the usual course in one case. He remarked that the ends of the nerves united by granulations. Incised and partially divided wounds healed like those where the division was complete, and the functions suffered little from them. If a portion of nerve was removed, the nerve healed in the usual way; the long time which the healing required, has been the cause of many doubting its real occurrence. Mr. Swan even doubted himself the restoration of the functions, until the following experiment convinced him of the accuracy of the fact. A portion of nerve leading to the leg, was removed from a horse which had been lame for two years; for six months from the time of the operation, the animal was quite healthy, but soon after it again became lame. On killing the horse afterwards, it was found that the nerves had united. In another experiment, quite new nerves were found, one which led to the ham, and another to the part where the fibular nerve is situated in the human subject. Ligatures produce the same effect as the division of the nerves. On the application of a ligature on the nerve, coagulable lymph is thrown out, and when the ligature comes away, the nerve re-unites, and the functions become restored. Richerand confirmed this fact by experiment. Soemmering and other anatomists have also advanced the opinion, that the substance of the brain may be also regenerated.

Flourens, in his experiments on the nervous system, has denied the accuracy of this opinion. The error, he thinks, has arisen from the great expansion of the cerebral substance after the infliction of wounds on it. This swelling after a few days disappears, and then it may be very clearly seen that no reproduction has taken place. The functions, it is true, become restored, but this does not depend on the reproduction of the parts. Experiments have also been tried on the reproduction of the spinal marrow. Arnemann divided the medulla spinalis of a dog in the lumbar region, and, after the lapse of eight weeks, the dog was able to walk. On killing the animal, it was found that the divided parts had reunited, but by what he conceived to be the nervous substance.

* Delpech Ricis Elementaire des Maladies Chirurgicales. Tom. I. p. 173.

† Magendie's Journ. Tom. I. No. 2, p. 122.

‡ Swan on the Local Diseases of the Nerves.

ROYAL INFIRMARY.

"Appointment of Dr. Cullen to a Surgeoncy in this Establishment."

None but the most ardent admirers of Dr. Johnson's principles of medical economy, will dispute the propriety of discussing the merits of a hospital appointment. It is, in its limited extent, as much a subject for public consideration, as the return of a member of Parliament for a borough, or the nomination of the holder of the "great seals." The duties which the elected has to perform are public; the electors themselves are but the public stewards of a common property, raised for the benefit of the poor; and the objects of the act of election are taken from the general mass of mankind, each of whom, as Paley admits, has a right to express his opinion of every measure in which the whole is concerned. The general assumption, however, that men volunteering to perform a sacred duty gratuitously, would discharge it faithfully, has been so long entertained, that to question the universally received fact, would, until very lately, have excited surprise, if not brand the sceptic with the ignomony of a malicious libel. As in other cases, custom became the law; all were satisfied, because all believed; the

right to inquire thus, was apparently annulled by desuetude; and its assertion, when provoked by abuse, naturally enough astonished the unthinking, and awakened the opposition of those persons who were interested in its further suppression. But it is now pretty well known, at least, to the readers of THE LANCET, that philanthropy is no longer what it was in the days of Howard and Penn; an unmixed emanation of love for the species, having strangely contracted certain impurities in the progress of time, which require to be rubbed off occasionally, to keep the machinery of pity in working order. The governors of hospitals, for instance, who form a large proportion of the philanthropy of the day, have been found by recent experiments to be quite anomalous "in their generation," and possessed of the most contradictory attributes. Benevolent, hard-hearted,—frugal, extravagant,—punctilious, and negligent, they constitute a species of comets in the social system, which claim exemption from all regularity of motion, and acknowledge no restraint but the whim of their own will. They call upon the community in the very accents of mercy to succour the agonies of disease, and hand over the objects of their compassion in the next moment to, perhaps, a medical executioner; they will strike a bargain for both with all the penury of an Israelite, and, like his prodigal son, throw away the savings in the purchase of a fancy pattern of blankets; they will at one time concentrate their whole attention on the burnishing of pannicans, and leave other less fragrant utensils to spontaneous detersion; they enact regulations about trifles, with all the formality of an Act of Parliament, and at another time abandon matters of real importance to shift for themselves; in fact, it would be difficult to collect into one view the capricious amusements in which these gentlemen now and then indulge. With one only, however, of the catalogue, have we any thing to do at present—the appointment of a surgeon to the Royal Infirmary. It would be a very unnecessary task to enumerate here the advantages derivable from the just exercise of the elective power of hospital governors. Like mercy, it may be said to act in a double capacity, by rewarding individual merit, and promoting the utility of charitable establishments; for on the reputation of their medical officers very often depends the accomplishment of the ends of such institutions. The knowledge of so apparent a fact could scarcely have escaped the notice of men otherwise so acute; but, true to the natural history of their genus, not the slightest account seems to have been taken of it in their decisions on the present occasion. With the utmost latitude of choice, they narrowed their views of discrimination, and found desert visible where it could never have been detected before. Not but the subject of their selection had pretensions of no ordinary kind, particularly when the theories of merit adopted by hospital electors are considered, the excellence of which the public are blind enough not to perceive. He was a namesake, for instance, to the great nosologist, and it was but fair to conclude, that he might turn out one himself; besides, every person knows the charm that resides in a name. More, he was of the same family; and the influence of lineal descent has been duly appreciated in appointments in Edinburgh for the last half century, and followed by the happiest results. Still further, he was really a professor, and, unless he was well informed, of course he could not teach, so that there could be no scruples on this head. 'Tis true, his audience consisted of but about a dozen of young gentlemen on their road to either of the "halls;" but then, if studious adults who had cut their "wise teeth," and were destined for higher diplomas, did not avail themselves of his instruction, the loss was surely on their side. Neither had he been much known to the public in those respects, which ensure a confidence in a practitioner's capability; but then he was young, and had no opportunity of signalising himself, both of which considerations were very judiciously taken into account by the electors. All these nicer subtleties of distinguishing abilities may be more thoroughly understood, when it is known that the governors actually passed by a certain celebrated operator, who, it is said, could perform the operation for lithotomy with his eyes shut, better than any man in Edinburgh could with the aid of Mr. Leslie's magnifying glass. Besides, this notorious operator had established a little hospital of his own by his dexterity; had the selection of all the more rare and difficult surgical cases amongst the poor of the city, with whom he was a great favourite. In fact, he was just such a man as most persons would suppose calculated in all respects for a dissector-in-chief to a national establishment; but governors are not to be diverted from their purpose by any such ostensive marks of efficiency as these, and consequently left them to reward themselves. The public, however, who are convinced by obvious reasons only, seemed to doubt the propriety of the decision—to inculpate the governors for their nice distinctions; but they ought to recollect, that there is no disputing about tastes, ever since the hero of Cambria settled that point by "kissing his cow."

SCOTUS.

Edinburgh, March 1, 1827.

OBSTETRIC SOCIETY.

On Wednesday evening, March 10th, a meeting of the "Obstetric Society" was held at the rooms of the Westminster Medical Society, in Sackville Street. Mr. Charles Clark was in the chair; and there were present, Drs. Ley, Granville, Merriman, Locock, Kerrison, and A. T. Thompson; Mr. Jewell, Mr. Stone, Mr. Sweatman, and three or four other gentlemen, whose names we could not ascertain.

The minutes of the proceedings of the Society, from its commencement, were read by the Secretary, Dr. John Ramsbotham, from which it appeared that, in the commencement of the year 1826, a meeting was held of certain individuals, who were of opinion, that it was requisite the practice of midwifery should be subject to some legal restriction. In furtherance of this object, it was agreed to form a society under the denomination of the "Obstetric Society." After being duly organised, this Society, in conformity with its avowed object, proceeded to address the respective medical corporate bodies, pointing out the evils which they believed to result from the indiscriminate practice of midwifery, and requesting to know how far the said corporate bodies were willing to co-operate in remedying the abuse, and whether they possessed the power and inclination of doing so.

The reply from the College of Physicians stated, that inasmuch as the act of delivery (in cases where assistance was required) was *merely* a manual art, it devolved upon the surgeon—that the diseases which occurred in the puerperal state, formed a part of the practice of medicine; and that, as such, the examination given to the members of their College, was sufficient to ensure their competency to practice in these cases.

The Council of the College of Surgeons, in answer to the communication of the Society, observed, that the Court were of opinion, they did not possess any legal authority, by which they could render it compulsory for candidates to undergo an examination on the subject of midwifery. And, lastly, the Right Worshipful Company of Apothecaries were also of opinion, that they could not legally exercise the power of examining candidates in midwifery, but they considered the subject as one deserving the consideration of the legislature; and if any enactment should take place, confiding the trust in their hands, they promised to discharge their duty with fidelity.

It would seem that the Society remained in a dormant state for some time after receiving these repulses, as nothing was done until the commencement of the present year, when it was agreed upon, once more, to address the three corporate bodies. The College of Physicians had nothing further to communicate; they referred the Society to their former reply, quite cavalierly. The Royal College of Surgeons, won by the gentle wooing of the Society, now stated, that they were willing to establish a board for the examination of candidates in the "midwifery department of medicine," provided such a proceeding could be legally sanctioned. The College of Surgeons prefaced this *gracious* communication, by remarking, that as it was desirable the members of the Council should be persons "well skilled in surgery," it was THEREFORE forbidden, that any member of the Council should practise pharmacy or midwifery. The Society of Apothecaries, in their second reply, regretted their *want of power, and said they should be happy to possess it.* The Obstetric Society thus finding that little could be gained from the corporate bodies, determined on addressing a memorial to the Secretary of State for the Home Department; in fact they had resolved on this course of proceeding previously to their second application to the "constituted authorities," provided nothing was done on their part. At a meeting held about ten days since, a sub-committee was appointed for the purpose of "drawing up" the memorial, and a meeting of the members was called for this present evening, in order to receive the said memorial.

The memorial was read by the Secretary; it consisted of a statement of the injurious effects alleged to result from midwifery being practised by ignorant persons. It stated, that whereas the legislature had given protection to the other branches of the profession, this was totally unprotected, and therefore it was humbly suggested to Mr. Peel, that the three medical corporate bodies should be required to take the subject under their respective jurisdictions, and institute examinations in midwifery.

The Chairman made a few observations on the objects for which the Society had been instituted. He disclaimed the idea of self-interest having actuated the members of the Society, for there was no intention of applying to Parliament for a distinct and separate charter; and the Society only desired to see some restriction placed on the practice of midwifery, as a public good; this measure being obtained, the Society would then cease to exist. They were not desirous of being separated from the profession, and regarded merely as a board of Men-midwives; they practised the profession generally, and midwifery only as a part of their duties. Mr. Clark here related an anecdote of Sir Astley Cooper, in proof of the dislike which men of general information in their profession felt at being con-

sidered to be acquainted with, or skilled in, one particular department only. He (Mr. Clark) was in attendance upon a young lady who was deaf; it was some years ago, and at that time when Sir A. Cooper had gained considerable repute in cases of deafness by puncturing the tympanum. It was proposed to consult Sir A. Cooper on the case of this young lady, and he (Mr. Clark) was deputed to write to Sir Astley; this he did, merely stating that he wished to meet Sir Astley at a certain hour, in a certain place, without speaking of the nature of the case. He received a reply from Sir Astley Cooper, stating his willingness and pleasure to meet him (Mr. Clark) at any time, but he hoped that the case on which he was now to be consulted was not an EAR CASE, *for he should feel it to be infra dignitatem to meet on such a case.*

Dr. GRANVILLE moved that the memorial which had been read by the Secretary, should be adopted as the sense of the Members of the Obstetric Society. He said, it was a notorious fact, that if a man wished to practice the medical profession and was not duly qualified, he might evade the penalties of the law by writing up over his door—ACCOUCHEUR. It was not long since, that a broken banker's clerk had applied to him for a certificate to practise midwifery. Dr. Granville said, that the anecdote which had been related by Mr. Clarke, reminded him of a similar circumstance that occurred under his own observation. He was in attendance upon a lady, who had ophthalmia, and it was proposed to call in Mr. Travers. Dr. Granville therefore met Mr. Travers in consultation, on which occasion he chanced to say that he was pleased his patient had had recourse to the opinion of such an eminent oculist; "I beg your pardon, Dr. Granville," (said Mr. Travers,) "*not an oculist*—BUT, A THOROUGH SURGEON."

A long debate ensued, principally with respect to the wording of the latter part of the memorial, which prayed that the corporate medical bodies should be required to annul or abrogate their by-laws as regards midwifery. Dr. Ley remarked, that as the corporate bodies possessed in themselves the right of framing by-laws, and also of dissolving them, it was useless to go before the legislature or a minister of the crown, and request him to use his influence in altering the by-laws of chartered bodies. He moved as an amendment, that it should be stated in this part of the memorial, that there was a necessity for a legislative enactment which should *enforce* the said corporate bodies to examine their respective candidates in midwifery. The amendment was approved by the meeting, and in this form the memorial was unanimously adopted. In the course of the discussion, the letter of Sir Anthony Carlisle, which first appeared in the Times, and subsequently in the THE LANCET, was alluded to, and the writer was treated with no great share of courtesy. Mr. Clark considered the letter as a libel on the talents and integrity of the mass of general practitioners; he said that it was worse than an attack on either purse or person, it was a scandalous letter, and teemed with falsehoods. Dr. A. T. Thomson stigmatised the letter as a *crazy, stupid* production, and therefore could do no harm; the source from which it proceeded would be remembered, and the public would estimate it accordingly.

It has come to the knowledge of the Obstetric Society, that on the occasion of the second answer being returned from the College of Surgeons, Sir A. Carlisle made a protest against it, which protest, however, was not allowed to be entered on the minutes of the Court. Sir Anthony, having been thus disappointed, thought proper to vent his spleen in the public papers. The members of the Society appeared to consider the attack as more immediately made upon themselves, and were highly incensed that Sir A. Carlisle should have availed himself of the information which he obtained in his official capacity, and have made it the basis of a slanderous attack—charging them with *vice* and *avarice*.

Mr. STONE read a long and (as it appeared to us) a very foolish letter, combating the assertions of Sir A. Carlisle; this letter he (Mr. S.) had drawn up for the purpose of sending to the Council of the College of Surgeons, and he moved that the letter be sent as from the Obstetric Society.

Dr. A. THOMPSON strongly objected to the letter; he said, that the Council of the College had nothing to do with the acts of a private individual, in which light he regarded the letter of Sir A. Carlisle, seeing that it was not written in his official capacity. There was no necessity for *reading* the College of Surgeons a *lecture* on midwifery.

We do not know whether this was intended as an allusion to the said Mr. Stone, being a *reader* of Lectures on midwifery; however, it was eventually resolved, that the letter should be forwarded to the Council. It was agreed upon, that a deputation, consisting of five members of the Society should wait on Mr. Peel, in order to present the Memorial. Some business with regard to the subscriptions having been disposed of, the Meeting was dissolved.

The Secretary remarked, that there were thirty-one members of the Society, each of whom had paid the sum of two guineas, the subscription for last year. Dr. CONQUEST fully approved of the objects of the Meeting, and was willing to affix his name to any memorial, but would not subscribe any thing except his *blessing*.

THE LANCET.

London, Saturday, March 17, 1827.

An advertisement on the wrapper of our last Number from Dr. James Johnson, announcing that another of his Medico-Fudgico Journals is about to be inflicted on the profession, reminds us that we have not quite finished with his last. It is indeed such an incongruous and "filthy stew," that we look into it with the greatest reluctance; but as its "hole and corner" readers, if we were not to assist their perceptive faculties, might mistake its unequalled dulness for the sharpness of wit, and its abortive attempts at reasoning as the finest efforts of human intellect,—we feel it a duty to render them all possible assistance. As our able friend Scotus has already exploded the gas which suspended Jemmy on his *reviewing* voyage (to the moon?) and consequently "let him down the winds to prey at Fortune," we are relieved happily from the necessity of wading through the first two or three hundred pages of his Medico-Fudgico hotch-potch. Lightened thus much of the burthen of our undertaking, we joyously pass on to his "remarks," as he calls them, on the report of a case of "aneurismal varix," taken from The Lancet, and which the "Scribe" has inserted amongst his "utilia." This case came under the management of Mr. White at the Westminster Hospital, was first described in The Lancet of *Aug.* 12, again mentioned on the succeeding week, and finally disposed of in the number of the *ninth of September*.

On this case the *Scribe* has thought fit to be critical; but experience should have taught him that it is "dangerous to meddle with *edged* tools." The public have not forgotten the ignorance and temerity he displayed in the affair of Magendie's *Journal de Physiologie*; and, unhappy wight as he is, he has on the present occasion committed himself yet more palpably. Let us examine what he has produced. First there is a note of interrogation affixed to the words "*radial* artery;" and he goes on to say, "*we* suppose the *brachial* was meant;" "no high bifurcation mentioned;" thereby intimating, that our reporter did not know whether the brachial or radial artery was wounded. The "*Scribe*" should recollect, that *a dissection* of the limb had been made previous to the writing of the report; that the *radial artery* was *found* to be *wounded*, and, *par consequence*, it must have been given off "higher up than usual," which is no uncommon thing. Was it necessary to tell *our* readers, that if the *radial* artery were wounded *at the bend of the fore-arm*, it must have had "*a high division*?" To have done so, would have been an outrage on their judgment. But he does not say that the *radial* was *not* the artery *wounded*. Oh! no. He knows that *it was*. "However, it matters nothing to him of the Quarterly."

Again, Mr. White "cut down upon the tumour:" "What kind of tumour?" asks the *Scribe*. We answer, "a large *pulsating tumour* at the bend of the fore-arm, occasioned by a *puncture of the radial artery*, in an attempt at *venesection*." This description of the *tumour* is to be found in the *first* account of the case in The Lancet, of *Aug.* 12, *which appears to have been entirely overlooked by the Scribe*; and the account published on *Sept.* 9, was only *a continuation* of what had appeared in the Number before named.

Lastly, says the *Scribe*, "the *report* leaves the man convalescing; but *we learn* that he has long since bid the world good night." The *report* was published on *Sept.* 9, 1826; the *Scribe's remarks, Jan.* 1, 1827, a sufficient interval for many hundreds of the Westminster and St. George's patients to bid the world good night.

We suspect that the *redacteur* of the labours of the "Associated Physicians & Surgeons," knows no more of aneurism than the *silk sheath* with which he pays off his wedded *rural*

scribes. If he did, he would not have ranked "circumscribed aneurism," and "aneurismal varix," and "varicose aneurism," as *three distinct* consequences of wounding an artery in phlebotomy. *Aneurismal varix*, and *varicose aneurism*, being *synonymous* terms for a pulsating tumour formed by the flowing of arterial blood into a suprajacent vein, in consequence of a communication established between them, by the vein having been transfixed, and the artery punctured, as occasionally happens in venesection. Such aneurism may be either *circumscribed*, or *diffused*, (in consequence of the escape of some arterial blood into the surrounding cellular tissue,) but it is no less an aneurismal varix, or varicose aneurism, on that account. Yet, wonderful to relate, the *Scribe* "made inquiries, and learned what *he* had expected, that the case was one of DIFFUSED ANEURISM!!"!!!

"The profunda superior was found *unusually* large, so were its radial and ulnar branches, affording an easy explanation of the cause of the repeated hæmorrhage, viz. the great supply of blood sent by these vessels into the anastomosing branches round the condyles."—LANCET, September 9th. The dissection of the arm was made only on the *second day* after the brachial artery had been tied; yet the profunda was observed "to be equal in size to the brachial;" and therefore it was *unusually* large, considering the short time that had elapsed after the operation—*only two days*. So much for the accuracy and value of the "Scribe's" "REMARKS" on this report. And now for the next, which he heads thus, "Prince Hohenloe in Panton-square," and then proceeds to criticise with wonderful self-complacency the report of a case from the HOSPITAL OF SURGERY, (LANCET, Vol. X. p. 852,) entitled "TUMOUR OF OVARIUM CURED BY COPIOUS BLEEDING;" and surely no unfortunate "Scribe" ever betrayed a greater portion of original and invincible dulness, than poor JEMMY in his "remarks" on this report. Obtuse as we have often found him, we did not suppose it was possible that even *he* could have fallen into such a truly ludicrous dilemma. The facts are these:—MARGARET B——, the subject of the report, had resided upwards of eight months within the walls of ST. GEORGE'S HOSPITAL, afflicted with what the officers of THAT celebrated Institution were pleased to denominate TUMOUR OF THE OVARIUM, at the same time stating, that it was as large as the *head of a child*! Eight months was she there without receiving any relief, when some kind friend recommended her to Panton-square, and in *nine days* the TUMOUR OF THE OVARIUM had *disappeared* "without any preternatural discharge from the vagina or rectum." Who can believe that the man of the FUDGICO failed to discover the real object of this report? Who can believe that he offered on it a serious critique? Yet such is the fact. Little did poor Jemmy imagine that he was pouring out his gull on the ACCOUCHEURS and BANDAGERS of St. George's, whilst ridiculing the miracles of Panton-square. Yes, this TUMOUR of the OVARIUM of *four years* standing, was cured by the HOHENLOE of the *Hospital of Surgery* in *nine days*. Would to God there was such a Hohenloe in St. George's. JEMMY JOHNSTONE, be on your guard; by J—s, we have *other traps* set for you!

We now pass on to page 266, and here, to our no small amusement, we find our medical Quixote fairly in the field of controversy, with no less an opponent than Mr. LAWRENCE—"Olympus to a molehill."

Jemmy appears to be greatly offended, because Mr. LAWRENCE has stood forward on behalf of the GENERAL PRACTITIONER, and because he illustrated in his Introductory Lecture, the absurdity of attempting to maintain a distinction between *Medicine* and *Surgery*. This is not the first time that JOHNSTONE has spoken in a CONTEMPTUOUS manner of General Practitioners, and pro-

bably it will not be the last; but he shall rue it before we quit him. Is it not most surprising that *he*, above all men, should give himself such airs, and endeavour to lord it thus mightily over the asserted insignificance of his professional brethren; *he*, whose door only a few short years back was thus labelled—Mr. James Johnson, *Surgeon*, and afterwards Doctor *James Johnson*, Surgeon? Yes, this creature himself, a surgeon, (at least he called himself such,) having obtained an *Aberdeen* or *St. Andrew's* diploma, which indeed any sweep in this town might have procured only two or three years since without difficulty, has now the insolent presumption to speak of the general practitioner as belonging to an inferior *grade*. In what manner did this fellow allude to these gentlemen in his Number for *April*, 1826, whilst giving an account of the meeting of Members of the College, at the Freemasons' Tavern? Mark well the language of this great St. Andrew's aristocrat, of this *Jemmy Johnson, Surgeon*. We quote from the above journal, page 581. "The meeting contained a very small proportion of men of age, note, or influence in the profession. The *great mass* consisted of the younger members of the College, *including* General Practitioners." And now, when Mr. Lawrence stands forward to assert their just and legitimate claims to public consideration and respect, this Aberdeen or St. Andrews Doctor, steps into the arena, seizes the gauntlet, and *accuses* Mr. Lawrence of attempting to produce equality. Hear the creature: "But the fact is, that Mr. Lawrence's scheme in medicine, is just what the scheme of Cobbett and Hunt is in society at large.—Equality." (Fudgico for Jan. p. 267.) True, Mr. Lawrence is anxious that there should exist such an *equality* of talent amongst medical men as would secure the public from the pernicious practices of ignorant pretenders, and that this *equality* should be regulated by the *scale* of a judiciously constituted College. But, surely, Johnson cannot be such an ass as to suppose that Mr. Lawrence is anxious for the establishment of that sort of equality which the above passage indicates! Does not old Fudgico himself *feel*, that the thing is impossible; or, that if it were possible, Mr. Lawrence is the last man who ought to seek it, and that Jemmy should be one of the first? *Equality*, indeed! how, in a moral point of view, can such a state of things exist, whilst there are men of such opposite mental faculties, or, rather, whilst many have no faculties at all? If Cobbett and Johnson were to commence the Spencean system to-morrow, Jemmy would be blacking the shoes, boiling the kettle, or grooming the horse of the Political Leviathan, before that day fortnight; and if he were obstreperous, these would not be the worst of his occupations! We are really at a loss to conceive *why* Johnson should speak so contumaciously of the General Practitioner. Mr. Lawrence, it will be recollected, in his admirable lecture, hinted at the establishment of a College, in which all should be examined "*without any regard to the present artificial distinctions*," which examination "*should authorise the individual to practise any or all parts of medicine, and none should be allowed to practise without it.*" We feel no hesitation in stating our most decided conviction, that this proposal, if properly executed, would prove the *ne plus ultra* of medical legislation; it offers, at once, a protection to the public, and a safeguard and honour to the practitioner; and the day *must* come, when it will be adopted. It is, however, a scheme exceedingly shocking to the delicate nerves of our sensitive *Aberdeen Doctor*, who exclaims with apparent horror, "thus, then, according to this Utopian plan, the *metropolitan physician and surgeon*, and the *village apothecary and accoucheur*, must undergo *the same examination*." Oh, dear! how very terrible this would be to St. Andrew's Doctors! He continues, "and consequently, must have the same

general and medical education! This is EQUALITY, with a vengeance!" The arrogance and stupidity here exhibited are truly disgusting. What does the creature mean, by "CONSEQUENTLY must have the same general and medical education!" Does it follow, that if the different candidates were competent to surmount such an examination as should establish their claim to be intrusted with the care of the afflicted, that their education must have been equal in degree, as well as in kind? Would not some, from their superior genius and industry, rapidly rise to the most exalted eminence, whilst others would remain stationary, and not advance in reputation a single point beyond the possession of their diplomas? Yet both classes, *merely* from the EFFICIENCY of their examinations, be equally entitled to the rank of gentlemen, and deserving of the public confidence? Assuredly they might. The quality and quantity of the test, are all we have to discover to protect both the public and practitioner. And why should not "the village apothecary and accoucheur," (the *real* general practitioner,) be as strictly examined as the *metropolitan physician*? Are the lives of the country gentlemen, industrious farmers, enterprising manufacturers, and unsophisticated agricultural labourers, of less value than the tax-eating, half-smoked, half-suffocated, lazy drones of this overgrown city? Once more listen, ye country "draff," to what this Johnson says of you: "Now, if Mr. LAWRENCE had given himself the trouble to think, for one small minute before he spoke, he would have perceived that *such an* EQUALIZATION would place the necessary acquirements *above the reach* of a *great proportion* of GENERAL PRACTITIONERS in the country—and that it would *reduce* the said acquirements to too low a standard for those (such as Jemmy) who aspire to honours and distinctions in cities and in the metropolis." Now it is evident that he wilfully insults the provincial surgeons, or that he is grossly ignorant of their talents. Talk of the *acquirements* being *above* their reach, indeed! We can tell him that they are surpassed by no practitioners in this town, whether *pure* or *impure*; there is no shifting of patients with them, from surgeon to physician—from physician to accoucheur—from accoucheur to oculist—from oculist to dentist—from dentist to electrician—from electrician to fumigator, &c.; but they alike skilfully execute the most formidable of surgical operations, and treat with the greatest judgment the most appalling of those diseases denominated *medical*. We shall return to this subject. JOHNSON'S affected and impotent sneers at the liberal, enlightened, and philosophical opinions of Mr. LAWRENCE, cannot be more appositely compared than to the blinkings of an owl at a noon-day sun.

Mr. CHARLES BELL, it is said, has given the pupils of Windmill-street the *coup de* STANLEY. A prospectus was issued with his name, and, as a Correspondent writes, "conveying the tacit understanding (implied contract) that he would as usual take an active part in the conduct of the school;" but "the fact is," continues the writer, "the whole of his instruction has been limited to eight or nine lectures during the last course, and the pupils have been taught what to expect in the present, by the cool annunciation that that gentleman has taken his leave." We believe that the fact is so, and are sure that such conduct needs no comment. The school has long been on the wane, but while Mr. Bell continued there, the struggle of existence might have been maintained;—

"Nondum illi flavum Proserpina vertice crinem
Abstulerat,——"

at length, however, the fatal lock is cut.

"*Review of some of the Surgical Cases which have lately occurred in the Royal Infirmary of Edinburgh.—A Clinical Lecture delivered to the Students of Surgery in that Institution, on the evening of Monday, 26 Feb. 1827.*" By GEORGE BALLINGAL, M.D. F.R.S.E. Fellow of the Royal College of Surgeons, Surgeon Extraordinary to the King, Regius Professor of Military Surgery in the University of Edinburgh, and one of the Surgeons to the Royal Infirmary. A. Balfour and Co., Edinburgh. Feb. 1827.

DR. BALLINGAL has plunged from the composure of the professor's chair into the turmoils of controversy. He is not satisfied with the dignities and emoluments of his multifarious offices, but will prove himself worthy of their enjoyment; he is a warrior, and will enforce, if he cannot command by more gentle means, submission to his opinions. He is above the patience of reproof—beyond the commission of error; his pride has been hurt, and by his own right hand alone shall the insult be avenged. To this end he has marshalled all the energies of his acute mind into disposable order for the conflict—summoned from the herald's office his manifold honours around him—arrayed himself, in short, in a mail of pompous appellations, and thus gaily bedecked, sallies forth, like an Eastern prince, in all the majesty of titular power to crush a shadow. His tender sensibilities for the reputation of his "House," are at least entitled to respect: and as he has chivalrously condescended to commit himself to the fortunes of proof; by his own evidence shall he be tried. In order to give him all the advantages which can be derived from his own statements, they shall be transcribed without curtailment, as far as they bear on the questions at issue; and first on the list appears the case of David Turnbull, for he has taken them in chronological order from THE LANCET, lest his intentions might be mistaken.

"In this review I am desirous to include a case of some interest which occurred in the early part of the course, previous to my assuming the duties of the chair, and which is closely connected with the subject of strictures, on which I had the honour to address you last meeting. The case to which I allude is that of David Turnbull, a boy who was admitted into the hospital, labouring under an impediment to the excretion of the urine, the result of an injury on the perinæum, and accompanied with a tumour in that situation, formed, as I conceive, by a deposition of lymph in the cells of the corpus spongiosum urethræ. The prominent symptoms, as you will recollect, were some degree of fulness, and tension in the region of the bladder—inability to expel the urine in a full or continued stream, while at the same time it dribbled away incessantly, constituting a complete enuresis. Having, after repeated trials, failed in making my way through the injured part of the urethra, either with the gum elastic, with the plaister, or with the metallic bougie, I was induced to attempt a cure with caustic. This was conveyed down to the stricture in the usual way, but after a few applications of the armed bougie, it became obvious that the caustic was making little or no impression on the stricture, while the constant stillicidium urinæ was liquifying the nitrate of silver, and diffusing it over the anterior part of the canal, which I stated, at the patient's bed-side to the pupils, as my reason for discontinuing the caustic. I afterwards proceeded to cut through the indurated portion of the corpus spongiosum, and to lay open the injured part of the urethra on the point of a director, with a view of introducing a catheter into the bladder; but, after a protracted search, I found it impossible to discover the posterior orifice of the canal. I therefore ordered the patient to be conveyed to bed, expecting, that after suppuration should be established in the wound, I would be able, by observing accurately the point from which the urine was discharged, to find out the opening of the urethra, and to lodge a catheter in the bladder. The wound, with this view, was ordered to be poulticed, and the boy directed to grasp the penis and compress the urethra, for the purpose of forcing the urine through the wound. Compression was also applied on the anterior part of the canal, by means of a ligature passed round the penis; but this could not be borne so as to produce the desired effect. The urine from the first came almost entirely by the natural orifice of the urethra, and the wound, contrary to my wishes, healed by the first intention, leaving the patient's condition very little bettered by the operation, except in so far that the tumour in perinæo was almost totally re-

moved by the absorption consequent to the incision, and the patient stated that he was sensible of the urine coming farther forward, without interruption, than it had prior to the operation. Under these circumstances I resumed the use of the common metallic bougie; but the little patient, dissatisfied with the result of the operation, became irritable and discontented, he behaved impertinently to the nurse, was reprimanded, and in consequence left the hospital."

Divested of the parental colouring of the author, the case, upon his showing, stands thus:—A boy is brought into the hospital with a tumour in the perineum, distended bladder, and partial retention of urine. This tumour the author believes to have been formed by a deposition of lymph, which lymph he subsequently expects to remove by establishing suppuration in the part, and, as a proof of the correctness of the opinion, refers to the partial disappearance of the tumour by the absorption consequent to the incision into the urethra. With this very rational view of the case, (although an *ex post facto prognosis*,) and with the preceding urgent symptoms demanding immediate interference, what does he do? Why, instead of carrying his own views into execution by the scalpel, he spends days—weeks, in the employment of all sorts and sizes of bougies, elastic and metallic, as he calls them; next tries caustic, the failure of which, according to his own explanation, he ought to have anticipated or guarded against; and having thus tried every means but the one deemed competent to a cure, he places the patient on the table, and fails in as favourable a subject as ever operator had to perform on. This, however, is not all. The urine, he says, from the first, passed almost entirely by the natural orifice, and the wound, contrary to his wishes, healed by the first intention. But did surgery afford no means for keeping a wound open? This question he in part answers himself, by enumerating the means which were adopted for that purpose. Compression was used to force the urine through the wound; the urine, it is admitted, did pass through it; it was poulticed; dossils of lint were introduced into it; it suppurated, remained open for weeks, and yet a full Doctor of Medicine of the University of Edinburgh has the extreme modesty to assure us, that a wound, after all this, healed contrary to his wishes, and that too by the first intention! The brass never shone clearer through the quicksilver of a counterfeit shilling, than the *truth* does through the whole of this ingenious fiction.

After informing us that the operation was not new to him, having performed it at Masulipitam (for be it known he has been "in the wars") and at Piurshill Barracks, he proceeds to the case of John Barclay.

"The next case of which I wish you to bear the facts in your recollection, is that of John Barclay, who was admitted with hydrocele of the right side of the scrotum. This man had previously been affected with the same disease on the opposite side, for which he had undergone the operation by injection. This, according to his own account, and as appeared from the extent of the cicatrix on the scrotum, was followed by extensive sloughing, but eventually terminated in a radical cure. On the 12th of December you saw him subjected to the same operation on the right side; after the fluid (urine and water) had been thrown in with a very moderate degree of force, and retained for several minutes, the orifice of the canula was opened for the purpose of giving it vent; a very small quantity of it, however, was discharged; and thinking that the injection had been unfortunately throwa into the cellular substance of the scrotum, I made an incision through the skin along the canula, with a view of giving it exit, a practice which, under similar circumstances, I would again adopt. No fluid, however, was found in the cellular substance, but a membranous cyst or bag, to all appearance a portion of the tunica vaginalis, protruded into the wound, and on puncturing this with a lancet, the greater portion of the injection was discharged; considerable swelling and inflammation of the scrotum followed; it ulcerated at the place where it had sloughed after the former operation; some small portions of what appeared to be dead tunica vaginalis presented at the wound, and after these were removed, the sore granulated kindly, and the patient was discharged cured. As to what may have been the cause of the obstruction to the discharge of the fluid, in the first instance, I have not

been able to satisfy myself, nor am I of course able to explain it to you. I am not disposed to enter into any hypothetical explanation, the reality of which it is impossible to establish. I should be sorry to hint at any explanation which might possibly mislead you, or which could be construed into an attempt to screen myself from any supposed error; but I will confidently repeat the assertion which I formerly made in regard to this case—'it was not an instance of an accident not very uncommon—it was not an instance of the injection having been thrown into the cellular membrane of the scrotum.' Had it been so, I could have found no difficulty in explaining it, and, if I know myself, I should have shown as little hesitation in avowing it."

As the writer has already expressed his opinion that the operation of injection should not have been performed at the time in this case—an opinion in which he is borne out by the results of the case, by the practice of most surgeons, and the testimony of the most respectable authors; he shall not repeat himself on that point here, but proceed to the statements before him. There was, then, no cicatrix on the scrotum of any extent. There was no sloughing after the former operation, unless the patient told a falsehood to the writer, who inquired particularly into the circumstance. No such occurrence was stated to have taken place in the account of the case by the operator, or was it reported in the hospital journal, farther than that a cicatrix had ulcerated, in consequence of the inflammation. But admitting that the sloughing had happened, (and it is evidently conjured up to justify the second sloughing,) what was the practical inference which ought to have been deduced from it?—to use every precaution to avert such a consequence again. Was this precaution adopted? Let the whole history of the case answer the question. But hear him out: he at one time thinks that the injection had unfortunately entered the cellular substance; acts upon that belief, by cutting into it; *presto*, he changes his mind, on making a cyst by the pressure of his own fingers; declares, now, that it was not an instance of injection into the cellular substance; and, as if to complete the climax of his contradictions, concludes by expressing his inability to explain the phenomenon, while he is, at the same time, cock-sure of what it was not. Spirit of Sir Gilbert Blane, was ever such a specimen of medical logic evinced before, by a Fellow of the Royal Society of Edinburgh!

Two cases of hydrocele form the next subjects of the author; but as they are of no further consequence than as attempts to parry the observations offered on them in The Lancet, the writer shall pass on to the author's grand defence of sutures, who writes thus on the subject, after describing the flap operations in which the stitches had been employed:—

"On the same day on which I operated on Kinmont and Jardine, I removed a carcinomatous ulcer from the posterior part of the neck of Donald M'Gelvray. In this case, as well as in the two preceding ones, sutures were employed to retain the divided integuments in apposition; adhesion however did not take place in this instance, and all the stitches except one were snipt across with the scissors at the first dressing, three days after the operation. Upon the employment of sutures, I am desirous of taking this opportunity of offering you my sentiments, because I am aware, that upon this point, I differ from some eminent individuals in the profession, for whose talents and experience I entertain the highest respect. Sutures have been particularly blamed for exciting that erysipelatous inflammation of the scalp, with which wounds of the head, however managed, are so exceedingly liable to become affected; but it does appear to me, that the complete closure of wounds by adhesive plaister, the irritation from the resinous matter in its composition, and the obstruction of the pores of the skin contiguous to the wound by its application, are more conducive to the production of erysipelas, than the employment of sutures; and my own experience goes to confirm this view of the matter. Those gentlemen who do me the honour to attend my lectures on military surgery, are well aware of what Messrs. Pilrae and Louis, and others, have written against the use of sutures in wounds; but I do think, Gentlemen, that (after twenty years hard work in the profession) I am just as well entitled to hold an opinion on this point as any one of these writers, and that opinion I can only be induced to

alter, in consequence of the results of future experience. There is little reason to change it, as long as I see such obvious good results from the use of sutures, as in the cases of Kinmont and Jardine, and none worse than in the case of M'Gelvray."

That adhesive plaster produced the effects ascribed to it here the profession was long aware, before the author's announcement. But what does he do to prevent these effects, and to find a substitute? first, by pricking the parts with needles, and then by using just as much of this irritating, erysipelas-producing, pore-obstructing, resinous compound, called adhesive plaster, as if the stitches had not been employed so at all; that instead of diminishing, he has, in fact, multiplied the sources of inflammation! It is true he refers to these stumps as triumphant answers to the impeachment of stitches; but what were the facts? both these stumps suppurated; one of them was unusually slow in the progress of cicatrization; but as the author believes that wounds which suppurate, as in the instance of Turnbull, may unite by the first intention, he might very consistently appeal to these stumps as specimens of that description of union. Surely if these stumps did well, which were every day dressed with adhesive plaster, this circumstance alone ought to absolve that substance from all imputation, if not entitle it to share the honour of the cure, exclusively ascribed to the stitches by the Regius Professor of military surgery!

Three cases of laryngotomy are next treated, in one of which the author apologizes for a hæmorrhage, &c., and then passes on to the case of Elizabeth Campbell:—

"In the case of Elizabeth Campbell, I had occasion to call your attention to a very unusual circumstance, the occurrence of colica pictonum, from the employment of white lead ointment, as a dressing to an extensive burn, which, owing to the rotation in attendance that took place at the death of Mr. Allan, was continued too long inadvertently; Dr. Hunter, conceiving that the ointment employed in this instance, was one which he is much in the habit of using in similar cases, that of the oxide of zinc. I have said that such an occurrence is unusual; because, although in the constant use of the preparations of lead, ever since I entered on the practice of my profession, this is the first occasion in which I have witnessed its injurious effects when employed as a dressing. And I find that my experience, in this respect, completely coincides with that of the late Mr. B. Bell, who declares that in all the experience which he had of the external application of lead, and, in many cases, particularly of burns, where he had known the greater part of the body covered with applications of this description for days, nay, for weeks, he did not recollect a single instance of any disagreeable symptom being ever produced by them."

This story, like the rest, explains itself. In one part of the defence we are told, that the lead was inadvertently continued too long—an admission necessarily implying a knowledge of the lead being employed in the case, and also a conviction of its possessing properties contingently injurious—while, in the next moment, this implied neglect is vindicated, as if the application of the lead were designed, by the experience of the author and of Mr. B. Bell, neither of whom ever saw any bad effects produced by this substance. If this were really the author's belief, there was no necessity for the plea of inadvertence, since the patient might be dressed, according to him, with lead until doomsday, with impunity. With respect to the rotation in the attendance, and the taking it for granted that the ointment used was composed of oxide of zinc, it is enough to observe, first, that Dr. Hunter had been in attendance on this patient long before Mr. Allan's death; and, secondly, that it is somewhat singular, that he could have supposed another practitioner to have prescribed a dressing used solely by Dr. Hunter himself, particularly as the white lead was the one generally used in such cases, in the house. One specimen more of the extraordinary logic of the Surgeon Extraordinary to the King, and we have done:—

"This patient (James Lees) was reported to have laboured under strictures. On his

admission, his whole urethra was inflamed, with a purulent discharge from its orifice; his distress was urgent; the symptomatic fever ran high; after having in vain sought relief from bleeding, general and local, from opiates by the mouth and injection, from the warm bath and gentle endeavours to introduce a catheter, a question arose as to the best means of affording him effectual relief, by an incision into the urethra, or by puncture of the bladder? Dr. Hunter very properly adopted the latter, because the urethra was inflamed through its whole course; because there was then no tumour in the perinæum, no appearance of urinary effusion, and no certainty as to the exact point where the stricture existed. The point where the catheter stopped, could indeed have been ascertained, but it was possible the cause of obstruction might have been farther back, nay, in the neck of the bladder itself. I therefore sanctioned the puncture from the rectum, which, in a great proportion of cases, I think the best mode of operating. The patient, however, died, though relieved for a time by the operation. His friends removed the body without an examination being made—a circumstance which we have reason to regret, on many accounts. I was anxious to know how far we had judged rightly, in preferring a puncture into the bladder to an incision into the urethra. This was a point, I admit, on which there might be a difference of opinion, as to which was the more eligible practice; but as far as the patient's life was concerned, I must think that the operation which was adopted, was at least as likely to have saved him, as the one which was omitted."

Some circumlocutions are necessarily omitted in the transcription of this case for want of space, but the substance is faithfully preserved. The puncture through the rectum, it is said, was preferred, because the urethra was inflamed throughout its whole extent. Granted. But how did the author ascertain this fact, for the all-seeing French speculum has not as yet reached the Royal Infirmary? O, there was pus at the orifice—*ergo*, the whole urethra was in a state of inflammation, though every tyro in the profession is aware that these circumstances are not invariably cause and effect in pathology. The urethra was therefore saved from the scalpel, by a supposition; but there was no hypothetical mercy in store for the bladder at all, which the author also suspected of inflammation. Was the danger, however, of cutting into an inflamed urethra, greater than the evil of forming a mutual communication—a sort of turnpike-gate between the rectum and the bladder? But why talk of cutting into inflamed parts at all; are they not cut into every day with impunity?—nay, is not cutting proposed, and known to be one of the most effectual means of relieving parts from inflammation? But what will be the reader's astonishment, on being informed, that this so much dreaded inflammation was not the slightest obstacle to cutting down on the stricture afterwards—that this condemned operation was absolutely performed in the presence of several witnesses, when there was neither tumour nor urinary effusion present; and that one of the surgeons of the Royal Infirmary, in his official capacity of reporter, positively states that such operation, to use his own phrase, was omitted!

SCOTUS.

Edinburgh, March 8, 1827.

Medical Botany, or illustrations and descriptions of the Medicinal Plants of the London, Edinburgh, and Dublin Pharmacopœias, with those lately introduced into Medical Practice. By JOHN STEPHENSON, M.D., and JAMES MORSS CHURCHILL, Esq., Surgeon. In Monthly Parts. Nos. 2 and 3, Feb. and March, 1826. Churchill, Leicester-square; Hodges and M'Arthur, Dublin; and Carfrae and Son, Edinburgh.

THE authors of this Medical Botany have amply redeemed the pledge which their first Number imposed on them. The plates are executed with still greater care, and the literary department is very satisfactorily sustained; so that we are fully justified in concluding, that the work to be completed will prove a valuable and ornamental addition to the libraries of medical prac-

titioners, and general readers. Each part contains descriptions of four plants; those delineated and described in the present, being *leontodon taraxacum, datura stramonium, spigelia marilandica æthusa cynapium, hyoscyamus niger, phellandrium aquaticum, helleborus niger,* and *lactuca virosa.* The plates are carefully coloured from nature, and the name, synonym, medical and chemical properties, uses, doses, natural history, &c. &c. of each plant, are so clearly described, that the unlettered reader will find no obstacle to stay his progress, and the learned little of botanical lore to wish for,—at least, as far as medicine is concerned.

One of the worthies of the Court of Examiners of the College of Surgeons, whose toddy-loving propensities are proverbial, sacrificed so freely at the shrine of the rosy god, on one of the nights appointed for the examination of candidates, that he committed the following "*slight mistake*:" "Pray, Sir, said Ursa Major, hiccupping, "Can, can, you t-t-t-tell me where the *Paddington canals* are sit-situated?" The affrighted youth stood aghast, but at length ventured fearfully and tremblingly to desire a repetition of the question; it was repeated as at first: "I presume, Sir," said the astonished candidate—"Don't presume here," growled the Examiner; "I think, Sir"—"Think, Sir? what right have you to think, Sir? Go on, Sir." "Why then, Sir, the Paddington canals are at Paddington." "Oh, you fool, hiccup'd Fuddle," you never dissected the eye in your life—I shall reject you." Here one of the Ten interfered, and courteously observed that Sir ——— had made a "slight mistake," the question intended being, "Where are the Petitian canals?!!"

Upon its being mentioned to Joe Burns that the letter written by Stone to the College was of a *caustic* nature, "Eh," said Joe, "no wonder, with the lapis infernalis!"

NOTTINGHAM.

MEETING OF MEMBERS OF THE COLLEGE OF SURGEONS.

At a respectable meeting of the surgeons of the town and county of Nottingham, convened on Thursday, 8th instant, at the Exchange Room by advertisement; Mr. Oldknow was unanimously called to the Chair.

The Chairman opened the proceedings by observing, that the purpose for which the meeting was assembled, was to take into consideration the London Petition of the Members of the Royal College of Surgeons, praying for an alteration in the present constitution of the College, and likewise to adopt such measures as should be thought the most eligible in support of that Petion.

Here the London Petition was read, after which the Chairman observed, there could be no doubt of the existence of the grievances there complained of, and for his part, he thought the origin of those grievances was fairly to be attributed to the circumstance of the President, Vice-Presidents, and Council, being a self-elected body, and that for life; and not responsible to the members at large for any act they might think fit to pass, of for the appropriation of their funds. That the by-laws and regulations recently adopted by that body, evidently evinced they were actuated by feelings of the most gross self-interest, rather than the advancement of the science of surgery, which they profess to uphold. That in his mind, it was in the highest degree unjust, when a young man had expended his money, his time, and his labour, in the acquisition of surgical knowledge, and had moreover made very considerable progress in that acquirement, that he should not be considered a fit person to present himself for examination before the officers of the College, merely because he had not studied at one of the favoured schools.—And that the "regulations" in regard to the unfitness of Country Hospitals, as proper schools of surgery, it is to be hoped, originated in a want of knowledge on the part of the Council, of the advantages of those institutions to the students attending them, and likewise the abilities of the surgeons, many of whom are equally zealous for the advancement of "sound" chirurgical knowledge as some of the members of the Council, and equally capable of communicating that knowledge. He should beg leave to propose the following Resolutions, which were duly seconded and agreed to:—

1st. That it is the unanimous opinion of

this meeting there are many grievances in the existing Charter of the Royal College of Surgeons in London, which most imperiously call for redress.

2d. That these grievances being so fully announced in the London Petition, we think it unnecessary to recapitulate them in the present instance, but we decidedly wish to express our opinion of their reality, and most strenuously to exert ourselves for their removal.

3d. That so long as the Council is self-elected, and permitted to make by-laws and regulations for its own government, it cannot be considered as a fair representation of the members of the College at large, and must ultimately tend to a monopoly injurious to the best interests of the science of surgery.

4th. That the denunciation of country hospitals (except under certain unattainable conditions) as unfit schools of surgery, appears to us degrading to the surgeons in attendance on these hospitals, and also injurious to the institutions themselves, by tending to lower the reputation of their surgeons in the estimation of the public.

5th. That the petition of the members of the College of this town and county, be transmitted to one of our members of Parliament, requesting him to present the same, and to support it to the best of his abilities; and that a letter be written to the other members, requesting their support to it also.

6th. That a subscription be immediately entered into to assist in defraying the expenses of the Petition to Parliament.

A vote of thanks was afterwards given to the Chairman for his handsome conduct in the Chair, and to Mr. DAVISON for his readiness in calling the meeting.

DR. BARRY'S PHYSIOLOGICAL LECTURES.

On the Powers that impel the Chyle along the Course of the Lacteals.

THE Lecturer, after having given a succinct view of the various opinions hitherto promulgated upon this subject, stated, that one of the greatest and most popular physiologists of the present day, M. Magendie, in enumerating the causes that move the chyle in the lacteals, had placed at the head of the list, "the unknown cause that produces its absorption." The power, then, that forces this fluid into the lacteals, and through the mesenteric glands, is unknown.

The chyle, Dr. Barry considers as colourless unfinished blood—as the carrying fluid in its unrefined state, loaded with the nutrient particles destined to become part of the individual. The current is from the surface of the bowels towards the heart, from the circumference towards the centre of a circle, as in the ovum and its fœtus. The power that moves the current, therefore, acts from without inwards. His reasoning upon his own experiments and observations, as well as upon those of others, had irresistably forced upon his mind the conviction, that all the circulating fluids of the living animal are moved by means of pressure. This pressure is either directly or mediately applied by certain organs of the animal, or by the expansion of gaseous fluids confined in certain hollow viscera. The heart and arteries are the chief agents of direct organic pressure. The respiratory muscles, of unresisted atmospheric or mediate pressure, by expanding a cavity around the point to which the centripetal current is directed. As to gaseous pressure, one of its species was illustrated in a former lecture, when it was shown that the small intestines of viviparous animals, distended by gas, envelop and squeeze the sponges of the ovum, forcing their contents into the branches of the umbilical vein, and through the embryo or fœtus.

The direct, or naked pressure of confined gas, is also employed by Nature in the higher animals, to squeeze the little sponges, the valvulæ conniventes of the chyliferous guts, soaked in the chyle which adheres to their shaggy villi, as the alimentary mass rolls over them from the stomach to the colon. This part of the lecture was illustrated by a simple, but, apparently, a very apposite experiment. A long phial (seemingly an Eau de Cologne bottle) was exhibited, about one-fourth filled by a blue water sufficiently acid to act upon a bit of zinc immersed in it. Through the cork by which the phial was closely stopped, a glass tube was let in, having a piece of sponge tied round the end within the bottle, so as to extend some distance both above and below the opening of the tube, but so as not to touch the blue liquid. The hydrogen gas, generated by the oxygenation of the zinc, passed freely through the sponge, and out by the tube, when the phial was held upright, and at rest; but when it was shaken, so as to throw the agitated liquid around the sponge, the gas, instead of passing out as before, now squeezed the contents of the impregnated sponge into the tube, and out through its external opening, in an uninterrupted column. If the apparatus were not again agitated, until the sponge had been squeezed dry, then the gas appeared in the tube, driving the blue water before it; but upon shaking the bottle again, the sponge became

changed anew, and the blue liquid again showed itself in the tube.

When the bottle was agitated often enough to keep the sponge constantly wet, then the whole of the blue liquid was successively forced up through the tube, without permitting the escape of a single bubble of gas, and this, though the sponge had never been in contact with the main body of the liquid.

Dr. Barry here related some very curious experiments which he had made upon the living animal, the results of which were analagous to these just stated. We regret that our limits will not allow of their insertion. The pressure of the gas within the intestines, then, is the power that forces the chyle into the mouths of the lacteals, along their tubes, and through the mesenteric glands. This accounts for intestinal absorption, and for the motion of the chyle after death, as well as for the swelling and sometimes bursting of the thoracic duct, between a ligature thrown round it and the gut. This gaseous pressure also accounts for the rapid disappearance of liquids from the stomach, and their almost immediate presence in the urine, though the duodenum be tied.

Some interesting proofs were drawn from comparative anatomy, that Nature avails herself of gaseous pressure, variously modified, in all the vertebrated animals, for the grand purpose of adding fresh matter to the individual, whether in its uterine or independent state. The gas of the intestines, said Dr. Barry, has never been hitherto looked upon as a mechanical power in the animal economy, and has been but very vaguely examined as a chemical agent. Its influence upon the motion of the blood in the abdominal and hepatic porta, and its importance in the pathology of apoplexy and other diseases, he reserved for discussion in another lecture.

HYDROCYANIC ACID IN DYSPEPSIA.

To the Editor of THE LANCET.

SIR,—Dr. Thomson asserts, that the only truth in my statement of no person having followed up his case and published an instance of stomach affection treated by Prussic acid when my collection of cases appeared, is the non-publication. But he thus allows all I desire. *After* the appearance of my book, cases were alluded to as having previously occurred, and I have no wish to dispute them. But priority of labour is always determined by priority of *publication*. I was therefore justified in saying, that at that time no one "had followed it up and published." The word "published" would alone have equally conveyed my meaning. I employed them both to convey but one idea, and did not write, followed up *or* published, but followed up *and* published; simply intending to say, that up to that time nothing was known to have been done by others.

With respect to my not having originally noticed Dr. Thomson's speculations as well as his case, I have made it a rule, in publishing reports upon medicines, to preface them by the previous *facts* only ascertained by others, because every medicine may be easily, and has indeed been, recommended speculatively in almost every disease. I had no other reason for passing over his speculations; because, *laying no claim to originality*, but merely to following up a fact, it was indifferent to me from whom I learnt that fact, whether, as was the truth, from a hospital dispenser who served the acid in mistake, or from others. To prove that I am not desirous of depriving Dr. Thomson of any credit, I shall have great pleasure, when reprinting my book, to break through my rule, and quote, as I did, not only the case of *heat of the tongue*, with the "*unexpected* effect" of the remedy, (to use his own words,) but also his speculations: though one of them—that the acid enables "the juices of the organ to be more slowly secreted," appears rather doubtful: while another—of the use of the acid as an "*adjunct to tonics*," falls infinitely short of the excellence of the remedy. But I must at the same time break through my rule by referring to Sprengel, Thuessen, &c. *who recommended it long before.*

I have many apologies to make for having occupied your pages with this subject. It has not been to support any claim, but to show that I do no wrong in not giving Dr. Thomson credit for originally exhibiting the remedy on scientific and inductive principles in dyspepsia, *when he exhibited it for another disease, and when the patient had actually no derangement of the stomach.* From this circumstance, and from dyspepsia not being in the list of affections mentioned in Dr. Granville's summary at the end of his book, as remediable by the acid, and from which only I made notes, I should never have thought of employing it in dyspepsia, but for the case cured by a mistake in the hospital, and did not observe Dr. Thomson's case of heat of the tongue till, having collected my cases, I searched that, as well as other works, for facts, previously to publishing. If, however, this correspondence convince Dr. Thomson that I had no wish to depreciate his merit, nor have the least unfriendly feeling towards him, but am anxious to forget every thing that may have been ungrateful to my own feelings on the

subject of Prussic acid, and if it excite attention to the power of the remedy in morbid irritability of the stomach, independent of or disproportionate to inflammation in the organ, and independent of affections of other organs, I shall sincerely rejoice.

I have the honour to be, Sir,

Your obedient servant,

JOHN ELLIOTSON.

Grafton-street, March 10, 1827.

HOSPITAL REPORTS.

GUY'S HOSPITAL.

CASE OF SLOUGHING OF THE GENITALS, WHICH TERMINATED FATALLY.

SUSAN POINTER, 22 years of age, a woman of the town, was admitted into the Hospital, on the 21st of February, under the care of Dr. Back, on account of jaundice, but, it being subsequently discovered that she had an affection of the genitals, she was transferred to the care of the surgeon, Mr. Morgan.

It was found upon examination that both of the labia pudendi were in a complete state of gangrene, they were black and dry, emitting a most offensive odour, and to a considerable extent up the abdomen, and also in each groin the skin was of a dark red colour. The poor girl stated that her residence was in the vicinity of the London docks, and that she usually had sexual intercourse with foreign sailors. She acknowledged, that she was accustomed to drink spirituous liquors, but more especially gin, to an extent which is almost incredible; at this same time, she partook of but little food, and was exposed to night air and the vicissitudes of weather. She could not give a distinct or satisfactory account of the origin of the complaint in the genitals; but it appeared from her description, that she first laboured under gonorrhœa, which had continued for about three weeks, when a small sore made its appearance on the right labium. The sore gradually increased, and within a few days of the poor wretch being admitted into the Hospital, it assumed the gangrenous state which we have described.

The appearance of the patient bespoke much internal disease—she was greatly emaciated, her features pinched, and countenance indicative of distress. The skin was of a deep yellow tinge, and she complained of tenderness when pressure was made over the hepatic region; she also laboured under a severe bronchial affection, denoted by the wheezing of her respiration, and a "stuffing cough." The pulse was small and very feeble in its beat, the skin was hot and dry, the tongue coated with a brown fur; she was thirsty, complained of pain and want of rest.

Mr. Morgan remarked that in such a case of extensive local mischief, with the addition (in all probability) of organic visceral disease, he could scarcely hope to save the patient. The only indication to be fulfilled in this case he thought was to support the vis vitæ; he directed five grains of the sulphate of quinine, one grain of opium, four grains of capsicum, and five grains of the carbonate of ammonia, to be given every six hours. Gin, or wine, and porter, ad libitum. The undiluted nitric acid to be applied to the part.

These measures were complied with; the poor girl, however, did not experience any benefit therefrom; she died early in the morning of the 25d, the second day after admission. The body was not examined.

TREATMENT OF BURNS.—MR. KEY'S CLINICAL LECTURE.

A few days since, Mr. Key delivered in the operating theatre, what he was pleased to term, "a clinical lecture—on burns." We know not what incentive the senior surgeon had to such a proceeding.—It may be, and we really have a shrewd notion such was the case, that Mr. Key was led to the attempt of proving, he at least knew something of the matter, in consequence of the remarks which appeared in THE LANCET, on the ordinary mode of treating burns at this Hospital. The said "clinical lecture" consisted for the most part of a garbled and confused statement of Dr. Kentish's opinions. Mr. Key at the same time omitting to state the source from whence they were derived.

Mr. Morgan's list of patients admitted on the 21st of February, presents but few cases of interest. Amongst the female patients there is a case of *irritable disease of the breast*, the particulars of which, as taken on the day of admission, are as follow:—

The patient is 15 years of age, of fair skin, and light complexion; she has never menstruated. The right mamma has been affected upwards of thirteen weeks,—the distinguishing feature in the complaint is the exquisite sensibility of the part, it is so morbidly sensitive that the patient can scarcely bear her clothes to press on it. When the breast is touched, excruciating pain is felt, which shoots from the breast

along the axilla, and extends someway down the arm; this pain is occasionally felt when no pressure is made. *There is no perceptible tumour in the breast,** but immediately beneath the lower edge of the pectoral muscle, where forming the anterior fold of the axilla, there is an enlarged absorbent gland, which is very tender to the touch.

The plan of treatment adopted in this case, has consisted in the application of leeches to the glandular tumour, the exhibition of an occasional purgative, (calomel and rhubarb,) and fifteen grains of the carbonate of soda, taken three times a day, with infusion of cascarilla. Subsequently, half a drachm of the carbonate of iron was directed to be taken three times a day.

The patient has experienced considerable benefit from the foregoing treatment.

ST. THOMAS'S HOSPITAL.

CASE OF SYPHILITIC IRITIS, ACCOMPANIED WITH PUSTULAR ERUPTIONS.

E. B., 23 years of age, of spare habit and unhealthy appearance, was admitted into the Hospital on the 1st of February, under the care of Mr. Tyrrell, on account of disease of the right eye.

On examining the eye, there was observed to be increased vascularity of the cornea, and also of the tunica sclerotica. The distinction between the two orders of vessels was strikingly apparent; those on the sclerotica were passing in straight lines, which, converging from all sides of the globe, formed at the base of the transparent cornea a complete vascular zone, having a brownish tint. The cornea was slightly hazy; the iris was much changed in appearance, being remarkably dull and muddy; at its ciliary circumference externally, was a tubercle of lymph, nearly of the size of a grain of wheat. The pupil was small and irregular, and was but faintly obedient to the stimulus of light, which was intolerable, and produced a flow of scalding tears. The patient stated that he had considerable pain in the globe of the eye, which also extended to the temple and forehead. The pain was most severe in the evening, and he had a distinct remission towards the morning. He said, that the right eye had been inflamed upwards of six weeks; previously to this, the left eye had been suffering, and he was treated for this at the Moorfield's Infirmary, but it was no sooner relieved, than the opposite eye was affected. He took pills, which made his mouth slightly sore. Both the upper and lower extremities were beset with eruptions of a *pustular* kind; the chest and abdomen were free. Some of the pustules were fading, but others appeared to be recent; they were small and distinct. The man acknowledged that he had a venereal sore on the frænum, about three months prior to his eyes becoming affected, and he took pills for the cure of the sore, but, according to his account, no ptyalism was produced.

Mr. Tyrrell, on examining the case, observed that it was a well-marked instance of syphilitic iritis, and he took an opportunity of pointing out to the pupils the leading characteristics of the disease, as the brownish coloured zone around the base of the cornea, the nocturnal pains, and so on. Mr. Tyrrell also observed, that he had most frequently seen eruptions of a *tubercular* kind, accompanying the syphilitic iritis, and that the deposit of lymph on the anterior surface of the iris seen in the present case at the ciliary edge, was a far less frequent occurrence than the deposit at the pupillary margin of the iris. Ordered,

Calomel, three grains;
Opium, one-third of a grain;

to be taken three times a-day. The extract of belladonna to be applied around the eye.

6. Not much improvement; there is slight ptyalism, but the pills have disturbed the bowels so much, that Dr. Elliotson to-day directed ten grains of quicksilver with chalk, conjoined with opium, to be taken in lieu of the calomel.

7. Ordered to rub in over the temple every night, fifteen grains of the strong mercurial ointment, mixed with two grains of opium.

10. The symptoms appear to be somewhat aggravated; the patient complains of much pain, and the vascularity of the eye is great. He has continued to rub in the ointment, and now takes the pills last prescribed, without the opium; the bowels are now free from disturbance.

17. The inflammation of the eye is rapidly subsiding, and the deposit of lymph on the iris has nearly disappeared; but the pupil is still irregular, and not dilated by the application of the belladonna. The patient now labours under profuse ptyalism. Omit the pills for the present.

24. The pupil remains irregular; but all the signs of inflammation are gone, and the eye is clear. Take a pill every night.

March 10. The eye continues quite free from inflammation; the pupil is still irregular, but is obedient to light; there is, however, considerable defect of vision. A slight

* This is a circumstance frequently observed in the peculiar form of mammary disease under consideration. Hence it is more proper when speaking of it generally, to say, *irritable disease of the breast*, rather than *irritable tumour of the breast*.

mercurial action has been kept up during the last fortnight. Mr. Tyrrell to-day remarked, that he did not anticipate any further amelioration in the case.

The patient will leave the Hospital in the course of a few days.

CASES OF PSEUDO-SYPHILIS,

Occurring in the practice of Regulars and Irregulars.

CASE 1. The patient, 32 years of age, was admitted on the 25th of January, under the care of Mr. Green, on account of swelling and redness of the integuments on the right side of the nose, with tenderness of the bone, and swelling of the mucous membrane.

The history which the patient gave of his case was as follows:—He had a large sore on the penis about eighteen months ago, for which he came into the Hospital, and remained several weeks; during his stay, he was kept under the influence of mercury for at least one month. The sore eventually healed, and he was discharged cured. At the expiration of five months, however, he returned to the Hospital with sore throat; he was again admitted, and treated for the space of two months. Mercury was again exhibited. In the month of August last, being a period of four months from his former sojourn in the Hospital; the throat once more became affected, and he was admitted into Henry's Ward, under the care of Mr. Tyrrell. On referring to the Ward-book, we find that he had at that time, ulceration of the soft palate and posterior part of the pharynx, and there was already considerable destruction of the soft parts from former ravages. The uvula was gone.

The patient said, that he had never been affected with eruptions on the skin, or with nodes on the bones. The nose was much swollen, being at the same time flattened and mis-shapen, and the skin on the right side was of a dark red colour; this appearance was of three weeks standing.

The treatment pursued in this case by Mr. Green, has consisted in the exhibition of five grains of Plummer's pill at bed time, with decoction of sarsaparilla three times a-day. The use of tepid fomentations, occasionally of leeches, and subsequently to these, the application of light poultices, have had the effect of subduing the inflammation of the nose, and the report made on the 7th of March states, that the part is entirely free from redness, although the nose still continues swollen generally, and the mucous membrane on examination evidently remains thickened.

CASE 2. This patient, a man about 27 years of age, was also admitted under the care of Mr. Green on the 25th of January, on account of pain and tenderness of the coverings of the cranium. Inflammatory affection of the conjunctiva, with considerable intolerance of light. The case is one of those of which we see almost daily instances—one in which the whole system has been *supersaturated* with mercury, under the treatment of the notorious Macdonald of the Kent-road, the man having, as he states, taken mercury upwards of nine months. The leading features in his complaint, are the intolerable pain experienced at night when warm in bed, and the tenderness of the periosteal covering in every part of the body, but more especially that of the head. Mr. Green directed the sarsaparilla, with Dover's powder, at bed time; but they had not been given more than ten days, when the patient, from some cause, left the Hospital suddenly. The affection of the mucous membrane of the eye was only a part of the train of symptoms consequent upon the general derangement of the system produced by the abuse of mercury. There were no distinct swellings in the bones, but the patient had had eruptions on the skin.

TO CORRESPONDENTS.

A "BOROUGH STUDENT" complains that Mr. LIZARS has not supplied the omissions in his work agreeably to his promise. We beg to call his attention to the subject.

"EXPLORATOR" is informed, that carbonate of lime does not decompose the sulphate of magnesia, unless heat be applied.

Communications bearing the following signatures have come to hand, of which we shall avail ourselves on the first opportunity:—VIGO—QUIZ—"AN ENEMY TO HOLE AND CORNER WORK"—THE DISINFECTOR OF A PUTRID OYSTER—NOLI ME TANGERE—A PUPIL OF Mr. DERMOTT—AN ENEMY TO THE HOLE AND CORNER SYSTEM—JAMES SNELL—A DISGUSTED MEMBER OF THE COLLEGE—CATACLUSM—LONDINENSIS—E. J. G.—A PUPIL OF THE LONDON HOSPITAL—JOSEPH RIDDLE—P. P.—X. Y. Z.—NON MEDICUS—A STUDENT—ROYAL COLLEGE *Notice!*—A. B. C.—A MEMBER OF THE COLLEGE—A.—MYSOTOXICOS—ARISTIDES—BLACK THORN—DAMON—R. E. J.—JOHN BULL—AN OLD PRACTITIONER.

ERRATUM.—Throughout Mr. Abernethy's Lecture in our last Number, where the name "*Morgagni*" occurs, read MASCAGNI.